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EQUILIBRIUM, INTEREST AND MONEY: THREE ESSAYS IN THE HISTORY OF ECONOMIC THEORY

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Equilibrium, interest and money: Three essays in the history of economic theory

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INTRODUCTION

The following essays share a degree of unity in outlook, a degree of diversity in subject matter treated, and, hopefully, also a degree of interconnectedness in meaning. The unity consists of the common stance they take toward theoretical problems. It is one underlying theme of each of these papers that theoretical insight into contemporary problems in economic theory is at least partially illuminated by scholarship in the history of economic thought. Accordingly, each paper involves an element of historical research. And what is more, the purpose of the historical work is not to establish historical primacy, or even always (though sometimes this is the case) to describe the actual evolution of doctrines. Instead, we at times take liberty with the grand historical march of ideas in order to rearrange thought by scrambling dates. Thus, for example, a comparison of the most modern views of the mathematical general equilibrium theorists and the more antiquated long-period equilibrium approach is essayed in Essay III.

Such an obvious violence to historical context is justified by the very nature of the discipline of economics. For it is peculiarly the case in economics, that while it continually lays claim to the status of science, with all of the procedural rules and social standing such a claim implies, it is also continually subject to internal disputes of a more philosophical, or even theological, nature. By this claim is meant something more than just the influence of precognitive "visions" that Schumpeter made so much of--although this is part of the claim. But more

than this (and contrary to Schumpeter's view of the history of economic analysis), economic's peculiar relation with its historical record (at least peculiar for a science) is that new work never seems to kill off the old. Parts of this record may lie dormant for a time, and some may experience longer-term growth than others, but all of it seems to possess an irreducible potentiality that can spring to life in a quite sudden and surprising fashion. Again, Schumpeter (1942) provides us with a clear example. Just such a rebirth of economic doctrine is chronicled in his piercing study of the influence of Marx.

Of course, this view does not replace the study of the actual context and meaning of the original doctrines, a pursuit that is more properly the domain of specialists in the history of economic thought. What it does, instead, is to lend contemporary relevance (and vigor) to this specialist work in the eyes of the profession as a whole. For inevitably when an economic research program appears to be proceeding along in quiet pursuit of scientific truth, some incubus of dissatisfaction is concurrently brewing in the wings. More often than not, the heretical brew that purports to tear down the old and bring in the new will also include an ample dose of ideas from dusty old books. Such is the progress of economic science.

If this is the theme that underlies the unity of the following essays, it is applied in a different manner in each. Essay I is an attempt to pursue the old economic tradition of borrowing concepts of equilibrium from the natural sciences (notwithstanding the view that Darwin borrowed his vision of evolution through competition from

economics!). In this case, we attempt a comparison of the most developed views of the equilibrium of physical systems, General Systems Theory, with the standard Walrasian approach to economic equilibrium. If such physical analogies have any meaning for economics, then the view of the economy as an open system yields some interesting insights into the problems of the Walrasian concept that still dominates so much of economic theory. In particular, drawing on Hayek's work on the foundations of economic equilibrium theory, we conclude that the crucial failing of the Walrasian approach is the lack of a good homeostatic explanation of equifinal equilibriums.

Essay II switches gears from general equilibrium theory to macroeconomics. In particular, this essay shows the usefulness of Keynes' own-rate theory of interest in giving analytical focus to his complex views on "the essential properties of interest and money." By tracing the development of this own-rates doctrine from a review of Hayek by Piero Sraffa in 1932 to Keynes' use of it in Chapter 17 of the General Theory, we pursue two puzzles. First, the own-rates framework offers a rehabilitation of the traditional neglect of Keynes' emphasis on monetary matters in the standard versions of Keynesian Macroeconomics. Secondly, the attention to the development of the own-rates framework sheds a shadow of suspicion over more modern attempts to interpret Keynes as a Wicksellian monetary theorist. Instead of considering monetary concerns as the source of disequilibrium disturbances to an otherwise smoothly operating "real" system (as in the Wicksellian tradition), we show Keynes to be an equilibrium theorist of a different color.

Finally, in the last essay we follow out the meaning of the notion of equilibrium involved in three views of prices, money and interest. In reviewing the Sraffa-Hayek debate, we get a clear glimpse of the traditional long-period approach to monetary theory as a disturbing cause. Then, by tracing Hayek's defense of this position to a confusion over the role of money and interest in value theory, we find a whole new conception of monetary equilibrium. This third conception (after the traditional long-period conception and Keynes' own-rates equilibrium) of monetary equilibrium, the "intertemporal equilibrium" method, turns out to be a variant of the Walrasian conception we started with in Essay I. Since each of these conceptions can be cast in terms of the own-rates of interest, it is illuminating to compare each on this basis. Our final conclusion is that Keynes' conception, lying somewhere intermediate between the other two, offers one method of providing the social level homeostatic mechanism that the Walrasian system was shown to lack in Essay I.

ESSAY I. IS THE ECONOMY A CLOSED SYSTEM? GENERAL EQUILIBRIUM AND GENERAL SYSTEMS THEORY

I. INTRODUCTION

The history of social analysis in general, and economics in particular, exhibits a persistent fascination with attempts to make social theory "scientific" (Keat and Urry, 1982). Comte[1] (1853) issued the original call for a "Positive Philosophy" of society that would develop general laws of prediction and control to enable the emergent industrial society of the 19th century to realize the same benefits of rationalization at a cultural level that science was already providing at the technological level. After Comte, this matter of fact approach to society is observed as social practice from the elaborate schemes of the Utopian Socialists in the 19th century down to the present in the form of the detailed records of the social accountants more appropriate to a computerized age. Likewise, in the more theoretical work of such diverse thinkers as Mill (1897), Marx[2] (1977, 1978), and Samuelson[3] (1963), there runs the common quest for scientific status.

It would, of course, be a mistake to see this cult of science as anything less than the pervasive feature of western civilization. Thorstein Veblen pointed this out 80 years ago:

> On any large question which is to be disposed of for good and all the final appeal is by common consent taken to the scientist. The solution offered in the name of science is decisive so long as it is not set aside by more searching scientific inquiry. . . . There are other, older grounds of finality that may conceivably be better, nobler, worthier, more profound, more beautiful. . . . But whatever the common-sense of earlier generations may have held in this respect, modern common-sense holds that the scientist's answer is the only ultimately true one (Veblen, 1906/1919, pp. 3-4).

It was part of Veblen's genius to work out some of the sociological implications of this exalted "place of science in modern civilization." He also had distinct views on the further question of exactly what standard of scientificity we are to subscribe to in this quest for respectability. Veblen was the unchallenged master of bringing the latest advances in biology, psychology, and anthropology to bear upon his unique analysis of society. Yet, as the unflinching analyst of the outmoded (pre-Darwinian) scientific preconceptions of economics, he would be the first to admit that science advances unceasingly in scope and conception. For although both Marx and Samuelson have called for scientific analysis in economics, their conceptions of science may in the end make them more different than alike.

It is in the spirit of Veblen's unorthodox range of scholarship that this essay will attempt to show how one of the latest mutations of scientific outlook, the work of the general systems theorists, might shed some light on the problems of orthodox economic equilibrium theory.

The plan of the essay is as follows. In Part II, I will survey the development of a new scientific outlook, general systems theory, that has arisen in the life sciences (like Veblen's "evolutionary point of view") in the last 30 years. Part III will be devoted to a brief review of the theory of general economic equilibrium, which seems to be the perfect example of an economic system in the sense of the general system theorists. In Part IV, I will point out that the fundamental anomaly general equilibrium theory presents from the general systems viewpoint, that of being both a closed and an open system, is a result of defects in

the framing of the general equilibrium problem from Walras on. In particular, I will argue that explicit recognition of the lack of any theory of the process by which Walras' equilibrium might actually come about and be maintained explains some well-known puzzles of the general economic equilibrium. Drawing on the work of F. A. Hayek, I will suggest that the problem essentially boils down to one of social-level facts concerning the processes by which knowledge and information are acquired in a social setting. Recognition of these shortcomings of equilibrium theory is forced upon us by the insights of general systems theory. With this recognition, though, comes a rationale for breaking out of the scholastic confines that the modern state of mathematical equilibrium analysis presents to the economic theorist without having to abandon its useful insights.

II. THE METHODOLOGY OF SYSTEMS THEORY

A simple example conveys the essence of the system theorist's view of scientific investigation. If asked to explain the operation of an internal combustion engine, there are two methods open to you. First, you could proceed to disassemble the engine and investigate separately the operation of the carburetor, pistons, valves, and camshaft. You would then reassemble the parts and reference your explanation of the engine by your investigations of the individual components. This method is called the analytic method. It is opposed to the holistic systems method where explanation of the engine would involve observation of the complete engine. In this approach, explanation of its operation would follow the path of the gasoline. From the tank, fuel is mixed with air by the carburetor and forced into the cylinders by the first valve, where it is ignited to drive the pistons which turn the drive shaft and ultimately the camshaft, so that the next valve is opened; and the process repeats itself (Muir, 1969, Ch. 2). Here, the engine is analogous to the system theoretic concept of an open system operating via negative entropy (the injection of fuel) with a simple feedback mechanism (the camshaft). It is the contention of the systems theorists that there are certain "complex interrelated wholes" which can be understood by the second procedure, but not by the first.

The case which gave rise to the modern version of systems theory is attributed to von Bertalanffy's use of the system concept to settle a long-standing dispute in the life sciences (von Bertalanffy, 1968, pp.

10-17). Stemming from certain developments in cellular biology (Phillips, 1976, Ch. 2) that appeared to contradict the mechanical application of physical laws to living beings, the organistic school of biology argued that the study of life was a "sui generis" phenomenon. This ran counter to the analytic approach whereby physical/chemical explanations were sought for biological phenomena. In claiming that the "emergent" properties of life were not reducible to physical laws, the organicists were rejecting the dominant reductionist research program of Enlightenment science. For the "vitalist" biologists, the attempt to reduce all science to the level of the ideal rational science, physics, was a misguided one.

von Bertalanffy attempted to solve this apparent methodological contradiction while retaining the validity of physical laws by using them to explain the uniqueness of living beings. His explanation centered around the distinction between closed and open systems. He began by defining a system at the most general level as "complexes of elements standing in interaction" (von Bertalanffy, 1968, p 33). Closed systems are those which are isolated from their environment. For this reason, given initial conditions and the laws governing the interaction of the system elements, a final equilibrium will be reached which is completely specified by the initial givens. This is the paradigm of physical and chemical experiments which operate in vacuums or closed containers. Physically, such systems seek the most probable state of maximum disorder through entropy since a more ordered state would imply the incomplete expression of the second law of thermodynamics and thus be improbable.

But von Bertalanffy noted that we observe systems in biology "which by their very nature and definition are not closed systems" (p. 39). Such living organisms are characterized by highly improbable states of organization from a physical standpoint. This seems to contradict those very physical laws that defined closed systems. It was von Bertalanffy's (1968, p. 39) insight to show that this contradiction disappears when it is realized that

> every living organism is essentially an open system. It maintains itself in a continuous inflow and outflow, a building up and breaking down of components, never being, so long as it is alive, in a state of chemical and thermodynamic equilibrium but maintained in a so-called steady state which is different from the latter.

Since this vital distinction was not made in the vitalist debate, says von Bertalanffy, it seemed that physical laws did not apply to living systems since conventional physics was designed to explain closed systems. He claims two major general conclusions are to be learned here. First, that the characteristics of an open system lead to the "principle of equifinality." By this is meant the fact that an open system can reach its particular steady state "from different initial conditions and in different ways." This is in contrast to a closed system where "the final state is unequivocally determined by the initial conditions" (p. 40). Mathematically, this distinction between equifinal steady states and unequivocally reached equilibriums can be shown to depend on the definition of a system as open or closed (von Bertalanffy, 1968, pp. 75-80).

The second general property he derives from this distinction is that it offers an explanation of "the apparent contrast between inanimate and

animate nature," whereby the physical law of dissipation to the most unorganized state of matter conflicts sharply with the biological evidence of evolution toward more complex forms. Again, appealing to the distinction of closed versus open systems, von Bertalanffy claims that closed systems obey this law of dissipation since there can only be positive entropy in such irreversible processes as closed systems represent. Open systems, though, can forestall this tendency to decay by imparting energy in the form of negative entropy and "may even develop toward states of increased order and organization" (p. 41).

From this amazingly simple and powerful distinction blossomed a revolution in biology that eventually grew into a scientific outlook that cut across disciplines (Rapoport, 1968). The rapid spread of the appeal of systems theory was the result of a coincidence of diverse developments of which I can only sketch the barest outline. Simultaneously with the growth of the systems view in biology, Norbert Wiener was extending his wartime work in mathematical self-guidance systems for armaments into a new science of information and control that he dubbed "cybernetics" (Wiener, 1948). A child prodigy with an early education that seems to have surpassed even that of John Stuart Mill in bizarre precociousness,[4] Wiener thought that his basic insight into "feedback loops" as the model of self-control had radical implications for thinking about such complicated "systems" as computers, language, sociology, and psychology.[5] In the more extreme flights of philosophical rhapsody of some of its proponents, cybernetics served as the symbolic manifestation of the complex information-oriented social control that was then beginning to

characterize so much of modern life.[6] It captured the spirit of the earliest enchantment with the hope of solving all social ills through the collection and management of vast data bases to be used for social engineering purposes.[7] In practice, its major impact seems to have been on the design and use of complex computer and technological systems (Maron, 1968, p. 547).[8]

For the growth of systems theory as a policy approach, this information-theory work pioneered by the cyberneticists was combined with operations research tools such as linear programming to set large-scale data collections into action. Also a product of the war effort,[9] linear programming found its perfect operational use in the applications of the prewar work of Leontief (1951) in data organization and Tinbergen (1956) in design of policy models. Leontief's organization of economic data into "input-output tables" was successfully married to Tinbergen's policy planning models utilizing the optimization techniques of linear programming. Despite Lord Keynes' early dismissal of Tinbergen's work as "black magic"[10] (Keynes, 1973b, p. 320), it was the political acceptance of Keynes' demand management policy recommendations that provided the fertile ground for the spawning of the "quiet revolution" in mathematical-statistical economics that we now know as econometrics (Fox, 1969).

Furthermore, as these initially economic-based system analyses gained recognition and (perhaps more importantly) public funding, they became the paradigm of applied social sciences to which other disciplines aspired. Consequently, attempts were made at data collection and

statistical analysis of a wide range of "social indicators" that were to parallel the impact of the purely economic accounts (Bauer, 1966). Combined with the postwar concern with "development," these "systems" of data collection and analysis spread geographically as well. Especially in the area of development aid to the Third World, the attempt to operationalize a system-wide view of society led social scientists throughout the sixties and seventies to construction and analysis of ever more complex data systems. Uses ranged from long-term macroeconomic policy planning to development of social accounting frameworks for analyzing development across cultures and over time. While most of these extensive pragmatic uses of systems concepts are not directly related to the abstract systems theory of von Bertalanffy and Wiener, they share a common outlook of complex interrelatedness with the former, conscious attempts at control with the latter, and appeals to span traditional categories of knowledge with both.

The interface where the abstract theorists of systems met with the social engineers is symbolically represented by the founding of the Society for General Systems Theory in 1954.[11] Growing out of the multidisciplinary milieu of the Center for Advanced Study in the Behavioral Sciences, this society was the brainchild of the biomathematician Anatol Rapoport, the physiologist Ralph Gerard, the economist Kenneth Boulding, and L. von Bertalanffy, a biologist. The latter recalls that system theory at the time "responded to a secret trend in various disciplines." This secret trend was toward "attempting scientific interpretation and theory where there was none and higher

generality than that in the special sciences" (von Bertalanffy, 1968, p. 14).

The original program of the society reveals the manner in which this secret trend was to be nurtured:

> The Society for General Systems Research was organized in 1954 to further the development of theoretical systems which are applicable to more than one of the traditional departments of knowledge. Major functions are to: (1) Investigate the isomorphy of concepts, laws, and models in various fields, and to help in useful transfers from one field to another; (2) encourage the development of adequate theoretical models in fields which lack them; (3) minimize the duplication of theoretical effort in different fields; (4) promote the unity of science through improving communication among specialists (von Bertalanffy, 1968, p. 15).

The work of the Society has been carried out in the General System Yearbook and the journal Mathematical Systems Theory. In general, the literature on systems theory has remained true to the society's original program. Work alternates from further developments of abstract, metascientific models of general interaction to trying to use these models to clear up difficulties in special fields or span traditional categories through applications of homologous models in each. Despite early hopes of widespread impact, though, the traditional barriers to cross-disciplinary research have proved more formidable than might have been predicted. This seems to be especially so in the more rarified pure-theory work of the various disciplines. Faced with more practical exigencies, the social engineers outpaced their theoretical counterparts in opening their eyes to other sources of knowledge.

This last distinction, between policy practitioners and professional theorists, has been especially marked in the field of economics. As we

have seen, the econometricians were pioneers in the systems approach to policy planning and this has led many of them to reach out toward other disciplines more recently.[12] Despite the practical economists' growing catholicism, though, the mainstream economic theorists of the last 50 years have taken little notice of outside influences. Instead, at the hands of the mathematicians that turned to economics from the 1930s on, economics has, if anything, narrowed its focus, at least so far as the problem addressed by the most abstract theorists, the problem of general economic equilibrium, is concerned.

I would argue that there were two reasons for this theoretical narrowness. First, as in the case of many scientific advances in history, the transformation of economic theory into a mathematical-based discipline that has taken place in the last 50 years has involved a large measure of work to translate old theory into a new language, here a mathematical one. Consequently, much of the work of general equilibrium theorists, for example, has been to prove the logical possibility of the existence of a state of equilibrium under ever more general assumptions. At its greatest degrees of abstraction, such a search became an end in itself. In fact, so abstract was the language of these topological investigations that the proofs often took years to be translated back to a more pedestrian level that would make them available even to trained economists. Thus, secondly, the mathematical economic theorists became isolated by their own success. Working with their given problem led to more and more complicated self-generated puzzles. So numerous were these self-generated puzzles that they had little need to look beyond their

system, much less at the economy itself, for further topics of research. The next section will be devoted to looking at this enthralling system in more detail.

III. THE WALRASIAN SYSTEM

In 1974, writing in the International Journal of General Systems, Kenneth Boulding expressed his dismay that economics had hardly noticed the development of general systems theory:

> Just why the economics profession has viewed general systems theory with such a massive indifference I really do not know. Like the physicists, the economists are so bound up within the elegant framework of their own system that they find it hard to break out into broader interests. Economists, indeed, may be a good example of a principle I have sometimes enunciated that "nothing fails like success" (p. 67).

Over 10 years later, it must be admitted that the situation is much the same.

In order to redress that massive indifference somewhat, the next two sections will be devoted to analyzing the relationship of general systems theory concepts to what Boulding thought was its most analogous field of study in economics, general equilibrium theory.[13] Along the way, we may gain some insight into what the terms of that dubious success that Boulding refers to really are and of what use the systems theorists can be to guide the further development of general equilibrium theory.

The now highly complex literature on general equilibrium theory in economics seems at first glance to represent a textbook case of a "system". The general formulation of the problem is as follows. The "economy" consists of large numbers of atomistic producers (with given techniques of production) and consumers (with given preference orderings of goods and endowments of resources). Each reacts parametrically to prices and attempts to maximize its constrained objectives of profit and

utility in a mutually consistent way (Bliss, 1975, Ch. 2). Since producers' profits depend on sale of their production, sales depend on demand, demand depends upon incomes, and incomes depend on the sale of resource endowments to producers, the individual agents' behaviors are complexly interrelated.

Historically, the first problem addressed by Walras in 1873[14] was "does there exist, in principle, a set of prices, one price for each good, that will simultaneously allow producers to maximize profits and consumers to maximize utility given the constraining interrelatedness between the actors?"

Combining his father's passion for the economics of exchange with training in mathematics and physics, Walras naturally saw this complex issue as a mathematical one.[15] Imbued with the spirit of the Enlightenment (Walsh and Gram, 1980, pp. 144-145), he saw his task as grounding the science of economics in the same bedrock upon which the Newtonian physics he knew so well was founded, the concept of <u>equilibrium</u>.[16] In so doing, he formulated the basic conceptual framework in which the theory of general economic equilibrium is still organized today, despite a century of improvement in the mathematical tools applied to it. Basically, this conception is that equilibrium in each market implies the equality of supply and demand. When supply and demand functions are defined with sufficient generality to include as arguments the resource endowments and the prices of all goods simultaneously, the complex interrelationships of production and consumption can be conceived of as a system of simultaneous equations.

Walras' feat was to formulate this system and derive such insights from it as his mathematical training would permit (Arrow and Hahn, 1971, p. 4). Having determined that the system contained a sufficient number of equations to determine the unknowns, Walras took "existence" for granted. Assuming a state of equilibrium, he could then go on to characterize an elegantly synchronized system:

> Equilibrium in production, which implies equilibrium in exchange, can now be easily defined. First, it is a state in which the effective demand and offer of productive services are equal and there is a stationary current price in the market for these services. Secondly, it is a state in which the effective demand and supply of products are also equal and there is a stationary current price in the products market. Finally, it is a state in which the selling prices of products equal to the costs of the productive services that enter into them. The first two conditions relate to equilibrium in exchange; the third to equilibrium in production (Walras, 1954, p. 224).

This integrative view of the economy as a complex system of interdependent markets and the insights that are to be had from such a "vision" have only been incrementally improved by the rarified analysis Walras' system has received at the hands of the pure mathematicians for the last 50 years.[17]

The history of Walras' system from 1874 down to our own time has been a curious combination of ups and downs on the tide of theoretical favor. Several lines of influence can be traced. In his own day, the vision that Walras formulated in his Elements was to inspire many diverse thinkers. Schumpeter (1954, p. 968), who considered him to be the greatest economist in the history of the discipline, would claim that his treatise represented the "Magna Carta of exact economics."[18] One line of influence that would run through Wicksell, H. L. Moore, Schumpeter,

Leontief, and J. R. Hicks down to much of present welfare economics, international trade, monetary theory, and growth theory is concerned with elaborating the economic content of this Magna Carta. This path to modern economics, though, was not a direct and unwavering growth of influence.

In the English-speaking countries, Walras' Elements remained a little-read book for decades and was not even translated into English until 1954.[19] The path of its influence in English-speaking circles took a circuitous route. Having been eclipsed by the dominance of Marshall for 60 years, the reentry of discussion of general equilibrium theory was most influenced by the publication of J. R. Hicks' Value and Capital in 1939.[20] Interestingly enough, Hicks' introduction to Walras was through Pareto, whose Manual he was assigned to teach by Lionel Robbins in his first year at the London School of Economics (Hicks, 1982, Ch. 1). This lapse of 65 years of attention within the Anglo-American world was not helped as it might have been by a knowledge of Wicksell since his work, as well, remained neglected until Kahn translated it in the thirties. Instead, it was through the enormous influence of Value and Capital that what Hicks called "the method of General Equilibrium" has become a pervasive influence on modern economic theory. In both its microeconomic specification of the individual actors via indifference curve analysis and its system-wide "temporary equilibrium" model, Value and Capital set down the foundation for most of what is taught in a modern principles of economics class: consumer theory, production theory, and (Hicksian) Keynesian macroeconomics. E. Roy Weintraub argues that "from this

perspective, it can be suggested that the decade of the 1930s saw the degenerative Marshallian program replaced with two competing programs, the macroeconomic and microeconomic, or the Keynesian and Hicksian" (Weintraub, 1979, p. 16).

But at the same time that Keynes and Hicks were brewing theoretical revolutions in England, another group of disciples was extending Walras' work on more explicitly formal grounds. This second line of influence stemming from the Elements was to catch that wave of interest in mathematical formulations of economic theory that the English tradition had been opposed to at least since Marshall,[21] but which swamped economics beginning in the 1940s. The intellectual object of this second line was a concern not so much with the economics of Walras' system, but the analytics. In particular, the concern of a generation of mathematically trained theorists, culminating in Arrow and Debreu's (1954) proof of existence of a general equilibrium price vector, was to rigorously dress Walras' crude existence argument in more elegant mathematical garb (Weintraub, 1983). Since it is the work of these mathematical theorists that provides the modern version of Walras' formulation of an economic system, the results of their work must be mentioned to bring our specification of the general economic equilibrium up to date.

Using more sophisticated mathematics, modern general equilibrium theorists have set out more precisely just what conditions are sufficient for the simple static case to hold. Generally, it has been found that Walras' insights hold but his mathematics were not very elegant. With

the sparest assumptions of nonincreasing returns to scale in production, convex preference orderings, and continuity in actors' reactions to prices (Bliss, 1975, Ch. 2), modern theorists have shown that the existence proof holds, but significantly for us, in general the solution is not a unique one.[22] This means that given the same primitive assumptions, more than one set of prices can satisfy the general equilibrium solution. In its traditional form, the theory cannot specify which will occur.

Additionally, as a result of recent debates between neo-Walrasian theorists and the Cambridge school devotees of Sraffa's (1960) system, the sensitivity of the solution vector to the given endowments of the system has been more explicitly recognized. Frank Hahn's attempt to show that the Sraffa system is nothing but a special case of the more general neo-Walrasian model led him to the conclusion that any specific solution to the general system depends on a "unique history" of the system (Hahn, 1982). By history, he refers to the fact that endowments or income distribution (which are explicitly "givens" to the problem) affect the whole equilibrium price vector through their influence on demand patterns. Hahn's contentious style in a highly charged debate obscures this fact with his argument. C. J. Bliss, though, is less coy on the subject than Hahn. He forthrightly admits the fact that in general the solution vector is nonunique and critically depends on what he calls "the primitive postulates of the system" (basically, those assumptions given as data from without the economic system) (Bliss, 1975, pp. 25-37). Finally, it is Bliss, again, who has no delusions about the neglect in

even the most modern formulations of the system of any dynamic behavior of the economy. A rather extensive quote from him at this juncture will serve to introduce the very themes to which a systems-theoretical account of general equilibrium compels us.

Bliss urges the following strategy for the theoretical use of the neo-Walrasian's proof that existence of a general equilibrium is logically possible:

> It may seem more sensible to simply assume that equilibrium will prevail and to thus confine our investigations to the equilibrium state. We could regard the object of our investigations not as "the economy" but as "economic equilibrium" and we could attempt to justify this procedure as a useful starting point to what one might eventually hope to see realized in a complete account of the behavior of the economy, including a full specification of its disequilibrium dynamics. . . . The awkwardness of proceeding in this manner is that we are introducing assumptions of a rather special, perhaps even dubious, character (Bliss, 1975, p. 28).

From the standpoint of general systems theory, the attempt to formulate the economic equilibrium without reference to the underlying dynamics would seem dubious indeed if we conceive of the economy as an open system.

IV. GENERAL EQUILIBRIUM THEORY AS GENERAL SYSTEMS THEORY

What is the relationship of the economists' general equilibrium theory and general systems theory? In its traditional form given above, the existence of a general economic equilibrium seems to contradict the insights of von Bertalanffy. First, it is simultaneously conceived of as an open system that takes inputs from the environment (both physical and social) and transforms them into outputs to the larger social system, as well as a closed system whose final equilibrium (a vector of relative prices) is completely determined by its initial conditions (endowments, technology, and preferences). Second, it seems to contradict the systems theory view that the elements of a complex interrelated whole cannot be understood in isolation from the whole system. For it is the hallmark of this approach that the theory is "built up" from the analysis of individual behavior of the maximizing agents in isolation from the whole. This is the meaning of making prices parametric to the agents. The whole, then, is conceived of as some complex sum of the parts with the parts clearly dominating the analysis. In other words, partial equilibrium modeling of consumers and agents seems to apply the analytic method with particular force. Yet, the usual argument for the extra insight gained in going from this partial approach to the general equilibrium is usually seen to be more in the spirit of systems theory in that it purports to show how everything depends upon everything else.[23] How can general equilibrium theorists have it both ways?

This contradiction is explained by two interdependent failings of

the general equilibrium theory of economics which when recognized make Boulding's claim for the applicability of general systems theory to economics more understandable. These failings are the lack of an adequate dynamic underpinning to explain the process by which such general equilibrium prices will be arrived at and maintained, and the general neglect of the knowledge assumptions necessary for such an equilibrium. We will take up each in turn and then try to show how they are related to each other and to the work of the systems theorists.

As should now be clear, the main focus of the development of general equilibrium theory has been on the specification of equilibrium states. The question of the conditions necessary for the equilibrium prices so defined to come about from any set of initial conditions has progressed little since Walras' own formulation. Walras resorted to a fanciful story about a "tatonnement" process whereby some fictional "auctioneer" would call out prices to the economic actors. The auctioneer then compares supply and demand for each good given to him by the agents in the form of tickets ("sur bons") and raises the price in markets with excess demand and lowers it in markets with excess supply. Only when he has stumbled upon a set that would allow the simultaneous equilibrium of supply and demand on all markets to occur, would he allow actual trades to take place (Walras, 1954, p. 172).

In the modern literature, the investigation of the conditions for "getting into equilibrium" has been subsumed under the mathematical rubric of "stability" of the system.[24] Again, although the mathematical techniques for defining the conditions of stability have advanced, the

schema still belongs to Walras. Bliss is candid on this topic as well.

> The investigations that have been undertaken into the stability of general equilibrium have admittedly been largely confined to investigating the stability of the Walrasian tatonnement--the adjustment process under which no actual trades take place until the equilibrium prices have been attained (Bliss, 1975, p. 28).

Thus, while all of the elements of the proof for existence of equilibrium prices depend analytically on the character of the individual actors in the system, the method by which the prices are assumed to occur has been seen as involving something over and above these individual actors. Bliss (1975, p. 16) quotes Koopmans (1957, p. 179) on this: "If, for instance, the net rate of increase in price is assumed to be proportional to the excess of demand over supply, whose behavior is thereby expressed? And how is that behavior motivated?" It is here that the system more faithfully resembles the system's theoretic claim of the whole of a complex system adding up to more than the sum of its parts. Stability concerns render the apparently most rationalistic and reductionist of all social scientific models dependent in the end upon an inexplicably social- or structural-level concept. But here that concept is anthropomorphically transformed into a being of such fantastic omniscience that it resembles Hegel's ideal spirit more than rational economic man!

Of course, it would be misleading to suggest that the general equilibrium theorists themselves have not been aware of this failure of general equilibrium theory. In the tradition of general equilibrium economists, Hicks noted this problem explicitly. In Value and Capital, Hicks states that the problem of "trading at false prices" could lead to

an unstable equilibrium of the system (Hicks, 1946, pp. 127-129, 245-272, 333-337). But instead of retaining Walras' tatonnement process, he opted for a scheme whereby equilibrium analysis is confined to an elementary time period, the Hicksian "week," during which stocks of production are given and demand determines prices "temporarily." This "temporary equilibrium" was a bow to his interpretation of Keynes' (1936) view of a monetary economy as a system in which the tatonnement process would be so time consuming that conditions would change before equilibrium could be established. During a Hicksian week, prices are not allowed to change within the week but only at the start (Monday) of the next week when expectations and plans are revised and new conditions prevail in the market (Hicks, 1946, pp. 131-140). His analytical argument, though, was deficient in rigor, essentially assuming that since "we may reasonably suppose that the transactions which take place at 'very false' prices are limited in volume" (Hicks, 1946, p. 129),[25] we can assume prices don't change within a week.

It was this analytical looseness that set the stage for the modern mathematical treatment of tatonnement in the work of Samuelson (1947b). Essentially, Samuelson's work involved specification of the underlying dynamic properties that were necessary to ensure that small movements away from an equilibrium would result in reestablishment of the equilibrium. From Samuelson's work stems the whole modern treatment of stability. Firmly within the <u>analytic</u> tradition, the problem is conceived as trying to use differential equations to more rigorously specify the mathematical properties of the auctioneer process that will

ensure stability (Weintraub, 1979, Ch. 2). The only new economic insight that seems to have come out of this work is that even utilizing the fictional tatonnement process the requirements for stability are very restrictive. Weintraub (1979, p. 33) sums up the results of this highly technical literature with a theorem:

> For the linear tatonnement system . . . the equilibrium is asymptotically stable, if the excess demand functions are continuously differentiable, are homogeneous of degree zero in prices, satisfy Walras' Law, and exhibit gross substitutability.

It is noteworthy that these conditions are much more stringent than the sparse assumptions necessary for existence. Since it is hard to imagine an economy which exhibits gross substitutability in all commodities (no shoelaces allowed), the implication is that "instability seems to be a universal phenomenon in competitive economies, rather than an exceptional one" (Nikaido, 1969, p. 337). So what do we throw out: equilibrium theory (if equilibrium is not stable, what sense is there in pursuing it?) or the fictionalized process by which this alleged instability has been pointed out? Both Weintraub and Bliss seem to think the safest bet is to throw out the auctioneer. To Weintraub, this traditional approach to stability was "singularly maladapted to the sort of decentralized decision-making that lay at the center of the static ADM (Arrow-Debreu-McKenzie) concept (and) . . . could potentially distract general equilibrium theorists from more comprehensive investigation" (Weintraub, 1979, p. 35).[26] For Bliss, the stability issue involves "work which may turn out to be singularly unrewarding to a scholar who would like to arrive at the conclusion that his model is a stable one

which the economy will tend to approach" (Bliss, 1975, p. 28).

As we have seen, Bliss thinks the proper alternative strategy involves simply assuming equilibrium exists and concentrating analysis on "economic equilibrium" rather than "the economy." Weintraub remains less insulated within his own "elegant framework," but retains his faith in the general equilibrium approach, which he considers to be the "hard core" (in the sense of Lakatos) of orthodox economics (1979, p. 37). In the spirit of the Lakatosian view of a rationally progressing science, Weintraub argues that the future of economics involves subsuming "the elements of post-war monetary theory and macroeconomics" (1979, p. 37), into the protective belt of the neo-Walrasian paradigm. By so doing, he claims that the traditional division of macroeconomics and microeconomics will not be bridged; they will both become part of one coextensive research program (1979, p. 71). Interestingly enough, he makes an analogy at this juncture between von Bertalanffy's GST and general equilibrium theory:

> A general systems theory, like the neo-Walrasian ADM model in economics, is more than a particular structure to model a particular situation . . . neo-Walrasian general equilibrium theory is not a theory in the same sense as, say, 'the theory of the second-best' or the 'theory of demand'. . . . Instead, we are suggesting that general equilibrium theory, the kind of general systems theory that economists have developed, is the appropriate logic to investigate the compatibility between microeconomics and macroeconomics (1979, p. 73).

A "kind of general systems theory," yes, but not a consistent, well-posed one if von Bertalanffy's work has any relevance for economic systems. Ultimately, the fiction of the auctioneer and the tatonnement process of getting into equilibrium obscures the very dynamic elements

which confirm the insight that Boulding sensed systems theory held out to general equilibrium theory, but was being ignored. Further, this insight helps to explain not only the paradox of reductionism and holism coexisting in the general equilibrium setting, but also a further anomaly from the systems theory standpoint. How can a supposedly closed system admit multiple equilibria? Like the vitalist debate before it, these anomalies also disappear when it is realized that the economic system is not, indeed, a closed system, but an open one. As a subsystem within a total social system, it imports data from the larger society in the form of raw materials, technology, income distribution, and tastes. With these, it produces goods defined in quantities and prices which in turn enter into that larger social system but in a nonunique manner. Given the extreme generality and lack of institutional structure in the general specification of economic equilibrium, it is not surprising that it should be amenable to a number of solutions once it is filled with a bit more reality.

Unfortunately, it is at this point that both general equilibrium theory and the general systems view of society generally break down. In the case of the theory of general economic equilibrium, the specific context for this breakdown can be described as an insufficient theory of economic knowledge. All of the elements of the functions of a capitalist economy are critically dependent on the knowledge available to the individual acting agents. Even the most static analysis of firm supply and consumer demand depends critically on the presumption that producers and consumers have knowledge of market conditions in the form of prices

of inputs, relevant techniques of production, competitors' prices, and prices of substitutes. In the more explicitly dynamic contexts of investment and capital theory, it is the treatment of knowledge in the form of expectations that distinguishes all modern approaches from one another. In the yet more complicated setting of a general economic equilibrium, then, the knowledge available to individuals becomes all the more important for any adequate description of how such a system-wide equilibrium will come about. Yet, it is precisely this domain into which orthodox economic theorists have been reluctant to tread until very recently. And when they have, the results have been dramatic departures from the main stream neoclassical vision.[27]

But although this problem is just now being treated by the formal mathematical general equilibrium theorists, its implications for standard equilibrium theory were pointed out half a century ago by F. A. Hayek. An examination of his "Economics and Knowledge" (Hayek, 1937) provides an interesting glimpse into the depth of the knowledge problem for orthodox equilibrium theory.

Hayek's paper is concerned with "the role which assumptions and propositions about the knowledge possessed by different members of society play in economic analysis."[28] He points out that the usual descriptions of formal equilibriums are essentially "tautologies" and "can be turned into propositions which tell us anything about the real world only insofar as we are able to fill those formal propositions with definite statements about how knowledge is acquired and communicated." As evidence of this view, he points out that attempts to push economics

beyond these normal tautologies toward causal explanations of such behavior as business cycles, interest rates, or oligopoly behavior typically involve assumptions about "foresight and expectations."[29]

For Hayek, this is due to the fact that the analyses of equilibriums which are defined for given data are properly applicable only to single individuals as a consistent explanation of behavior.[30] "Actions of an individual can be said to be in equilibrium insofar as they can be understood as part of one plan." Only if the actions are based on one set of data and assumptions about knowledge and preferences of the person can they relate to the same equilibrium. But Hayek, in good Austrian tradition, emphasizes that these "so-called 'data'" are subjectively given to the actor and "not in any sense objective facts." Only in the subjective sense is there a logical a priori consistency of the tautologies we derive from these data.

It is this distinction between objective and subjective givens that forms the crucial link in Hayek's argument that the method of equilibrium of the individual "does not immediately admit of applications to the relations between actions of different people." The difference lies in the nature of the "givens" in a social setting as opposed to a personal one. If a general equilibrium is to be considered, it must involve two types of mutual compatibility of actions. First, there must be a compatibility of the different actions of the different actors in the system since "the plans of the one contain exactly those actions which form the data for the plans of the other." But there must also be a compatibility of these subjective plans with the objective data of the

environment. Hayek claims that it is confusion over these different conceptions of givens that is "at the bottom of so many of our difficulties." For in the equilibrium of the individual, we could unequivocally define the givens as any conception that the subject (subjectively) held when he made his plan of action. For Hayek, "only this subjective interpretation of the true datum made these propositions (of individual equilibrium) necessary truths." However, in the more general equilibrium setting, this concept of datum becomes a much more problematic assumption. Since the data for one actor consist partly of the plans of other actors and, moreover, all of these individual plans must in some sense correspond to a given physical/social reality, the concept of what the data are in defining a general economic equilibrium becomes much more problematic. "The data which now are supposed to be objective facts and the same for all people are evidently no longer the same thing as the data which formed the starting point for the tautological transformations of the pure logic of choice." For Hayek, there is an "insidious change of meaning" in the concept of datum between these respective situations that forms the basis of the difficulty in giving general equilibrium theory any meaning.

Basically, the data problem consists of the confusion in stating for the givens of our "system" to whom these data are given and whether everyone in society is given the same or different data. The question of to whom the data of our analysis are given involves the distinction of objective and subjective data already mentioned. When we define a general economic equilibrium, we take the physical, technological, and

taste data as given to the economist making the analysis. But, surely, this is different than the data given to the actors in the system. Hayek claims it is essential to clearly distinguish the two types of data since they lead to two different conceptions of equilibrium.

First, we might define equilibrium as the mutual compatibility of "the subjective data, given to the different persons, and the individual plans which necessarily follow from them." This mutual consistency of individual plans is separate from, and prior to, the further requirement that these mutually compatible plans are in agreement with the objective data which are given to the society. Hayek claims that, as opposed to the forward-looking, expectation-based equilibrium of the first type, for correspondence with objective data, "it would never be possible to decide otherwise than ex-post at the end of the period for which people have planned, whether at the beginning the society has been in equilibrium." One implication of this distinction is that the only "changes in data" relevant to the definition of a general equilibrium are those which change the expectations of the individual actors in some way such that they alter their plans. In this light, the famous assumption of "full knowledge" on the part of some past and present theorists[31] becomes a crude attempt to cover up this problem by assuming everyone knows everything.[32]

Hayek goes on to point out that the important element left out of traditional equilibrium theory is the crucial link between these two types of data. In short, how do economic actors acquire knowledge of both the objective and subjective data that cause them to change their

plans? This link between the subjective and objective data of the system brings us full circle to our original problem with defining a general economic equilibrium and its relation to systems theory.

In the context of the auctioneer problem, we can now see that this crucial theory of economic knowledge was subsumed there under an analytical contrivance out of touch with the individualist statement of the original problem. Since the Walrasian equilibrium of equality of supplies and demands must come about through the dynamic interaction of individual actors adjusting their own plans, both to the rest of the actors' plans and to some external data, we see that it is not possible to define the parts in isolation from the whole. This anomaly, from a systems theory perspective, falls away when we realize that it is a classic case of missing variables that cause the general form of general equilibrium models to yield nonunique solutions. Moreover, it seems necessary to assume that these extra variables that would enable us to tie down the solution must in general be social-level facts such as Comte thought social science should proceed from. It cannot be another dimension of subjective minds but some social mechanism that structures the process by which this knowledge acquisition comes about.

Hayek hints at this conclusion when he points out that the lack of attention paid to this knowledge problem by economists "would go far to account for the fact that pure analysis seems to have so extraordinarily little to say about institutions, such as the press, the purpose of which is to communicate knowledge. And it might even explain why the preoccupation with pure analysis should so frequently create a peculiar

blindness to the role played in real life by such institutions as advertising" (Hayek, 1937, p. 67).[33] In full accordance with the systems theorists' emphasis on the value of homologies to span the work of different disciplines, a complete system specification of a general economic equilibrium, one that included a social-specific role for knowledge acquisition (or control), could possibly clear the theoretical ground for that union of economists and other social theorists that has been called for so many times since Comte first proposed a science of society. This is one way of viewing Fox's (1983) work on a theoretical framework for social data and Boulding's (1945, 1981) attempts to formulate systems of knowledge transfers through grants economics and conflict resolution theory.

In a wider sense, the questions this analysis has revealed can be seen as an organizing node for the increasing dissatisfaction with orthodox equilibrium analysis that has cropped up in the economics profession in the last 20 years. Analysts attracted by the system theory approach are but one sect of a variety of heterodox thinkers active today. Although their motivation, politics, and methodology exist in a multivectored space, one predominating influence on the original impetus for these schools was a rejection of pure equilibrium analysis as sterile and useless. Nevertheless, when it came time to formulate new work, a complete divorce from equilibrium theory has not seemed possible. Without some kind of structure or centering, such necessarily vague views as "process analysis" or "social economics" seem to degenerate into storylike descriptions at best, or obscurantism at worst. I think this

loss of an "analytical engine" involved in the rejection of orthodox equilibrium analysis, the same engine which had given economics its however dubious claim to analytical superiority within the social sciences, explains the recent attempts by nonorthodox theorists to embrace some variant of equilibrium in their analysis.

A prominent example of this trend would be the latter-day Cambridge school. These theorists start their analysis, just like a good neoclassical economist, from an equilibrium framework. In fact, in a manner reminiscent of Hayek's critique of orthodox theory, this framework has been criticized as consisting of little more than ex-post accounting identities. But the growing number of adherents to the Cambridge approach conceive of this Sraffa price system as a superior system of equilibrium "tautologies." In terms of our analysis, one distinctive feature of this system that could make it more appealing than the orthodox variety of equilibrium theory is that it explicitly makes note of the necessity of specifying some extraeconomic social process to close the system. But this is not distinctive in itself since, as we have seen, the same interaction with the social environment is implied by a complete closure of the neo-Walrasian system.[34] What ultimately distinguishes the two approaches on this matter is the Cambridge theorists' insistence that the manner in which this admitted openness is to be closed must come through specification of the income distribution between profits and wages. Moreover, the fact that they conceive of this distribution as an antagonistic social process that is logically prior to price determination makes it totally opposed to the noncoercive image of

distribution as an extension of free exchange that forms the basis of the Walrasian tradition (Dobb, 1973, pp. 247-266).

For our purposes, the very fact that the Cambridge approach takes explicit cognizance of the social environment provides an example of a more thoroughgoing systems theoretic view of economics. That such a view calls for extraeconomic investigation points out that the ultimate significance of general systems for economics may be just exactly what its founders had in mind when they called for a bridging of disciplines. To break out of the vice-grip of pure equilibrium analysis involves explicit attention to the institutional details of the case. This is something that applied economists have always recognized. An explanation of the demand for oranges in California would be deficient indeed if it merely reproduced a supply and demand graph. The implication of the systems view of economics is that the abstract theorists of economics, also, have a responsibility to formulate models which acknowledge institutions in a nontrivial manner.

Essentially, what is required of economic theorists is an attempt to recognize their environment, not ignore it. It is useful to note in this context that the essence of von Bertalanffy's insight lies in a precise recognition of a system's boundaries and its interaction with the environment. The very distinction of a closed versus an open system depends on this recognition. In fact, one of the most influential system-sociologists in recent years, Niklas Luhman (1982), builds his analysis on the intricate boundary relations, both intersubjective and subjective-objective, that form the social environment. But since this

is an essay in system "economics," not "sociology," I propose to round out our critique of equilibrium theory with a final speculative look at the methodological implications of the view of equilibrium proposed above for economics. We do so by concluding with a brief look at the work of two of the 20th century's most original minds: Ludwig Wittgenstein and Piero Sraffa.

In his youth, Ludwig Wittgenstein spent his considerable intellectual powers in trying to fulfill to its ultimate end the positivist program of grounding language to empirical reality in strict logical terms. His Tractatus Logico-Philosophicus (1922), written in the trenches of World War I, was considered by the then towering figures of Western philosophy to have fulfilled this goal. Yet, later in life, Wittgenstein came to consider the whole idea of a logical relationship of language (and hence thought) with reality as a peculiar form of "philosophical disease" of which modern thought needed to be cured (Janik and Toulmin, 1973, pp. 202-238). In his later work, Wittgenstein argued that all of the most interesting aspects of language and thought were the ones which could not be so logically treated. Instead of trying to force the actual functioning of language into a narrowly preconceived positivistic framework, Wittgenstein argued that all of the most interesting aspects of language and thought were the ones which could not be so logically treated. Instead of trying to force the actual functioning of language into a narrowly preconceived positivistic framework, Wittgenstein came to believe that we should recognize it for the richly complex social enterprise that it is. Thus, his later work

attempts to show, through the analysis of "word games," how socially relative and contextual language actually is as a form of human activity rather than as "the petrified fallacies of reason" that analytic philosophy made of it (Heller, 1965, p. 228).[35]

I think it would be possible and informative, although beyond the scope of this paper, to draw a parallel between the transition Wittgenstein's thought took over his life with the transition that is now and has for the last century been taking place in economists' conceptions of the relationship between pure economic (equilibrium) theory and the economy it provides a picture of. A key figure in this story could turn out to be a Piero Sraffa. For, his interesting influence on Wittgenstein's reversal (Roncaglia, 1978, pp. 121-124) aside, Sraffa's work in economic theory provides an object lesson in how such a transition might take place.

Sraffa, like Wittgenstein, started his theoretical career in an attempt to show the consequences of making the dominant orthodoxy logically consistent with itself (Sraffa, 1926). He then came to reject that orthodoxy completely, at least by the time of his arrival in Cambridge in the late twenties. This is evident in his terse comments in the "Symposium on the Representative Firm" in the Economic Journal (Sraffa, 1930, p. 93) where, in reply to D. N. Robertson's criticism of his view of Marshallian economics, Sraffa states his position most clearly:

> We seem to be agreed that the theory cannot be interpreted in a way which makes it logically self-consistent and, at the same time, reconciles it with the facts it sets out to explain. Mr.

> Robertson's remedy is to discard mathematics, and he suggests that my remedy is to discard the facts; perhaps I ought to have explained that, in the circumstances, I think it is Marshall's theory that should be discarded.

If it is possible to make the metaphorical leap to "Marshall's Theory" as a symbol of the tension between equilibrium theory and reality, I think it is possible to make the case that Sraffa's later life work (Sraffa, 1960) represents an attempt to show how vacuous a pure, logically consistent, equilibrium system really is. For, like Hayek's characterization of the orthodox general equilibrium theory, the major calumny that has been heaped upon Sraffa's equilibrium system is that it is little more than a set of "ex-post accounting identities" and explains no real behavior of agents. "Well if so, so be it," Sraffa might say. "Now go look for the real social processes and institutions by which agents learn and are controlled in society to flesh out this bare positivistic system that is only one of many 'life forms' of society." If this speculation has any basis,[36] Sraffa, in a much more subtle and involuted manner, can be attributed with the same prescriptive intention that Hayek (1937, p. 68) claimed as the aim of his work:

> All I have tried to do has been to find the way back to the common-sense meaning of our analysis, of which, I am afraid, we are apt to lose sight as our analysis becomes more elaborate. You may even feel that most of what I have said has been commonplace. But from time to time, it is probably necessary to detach oneself from the technicalities of the argument and to ask quite naively what it is all about. If I have only shown that in some respects the answer to this question is not only not obvious, but that occasionally we do not even quite know what it is, I have succeeded in my purpose.

V. ENDNOTES

1. See Kolakowski (1968) for a modern treatment of Comte's thought and its influence.

2. The issue of Marxism and "science" has grown into an immensely complicated literature. All that can be safely said is that on any interpretation, Marx (early or late) was very concerned that his analysis be perceived as science. His well-known polemics on the distinctions of Utopian socialism versus scientific socialism and "vulgar" economics versus scientific political economy turn on just this point. While numerous citations could be made in this context, two that illustrate his life-long concern with scientific analysis are the discussion of political economy in the 1844 manuscripts (Marx, 1978, pp. 90-93), and the "Preface to the First Edition" of Capital (Marx, 1977, pp. 88-93).

3. This short paper evoked a comment from Fritz Machlup (1964) and a reply by Samuelson (1964), which make interesting reading and might be given as a prime example of the difficulties of methodological discussions. All of these are reprinted in Caldwell (1984) who has this to say about Samuelson's methodological position: "How could such a renowned theorist be such an awful methodologist? Was his problem that he paid too much attention to the writings of philosophers, or not enough? Is there no interface between the scribbling of methodologists and the practice of economics?" (1984, p. 137).

4. Wiener, like Mill, was the subject of experiments by a father with singular views on education. Perhaps instead of this resulting in a

nervous breakdown, as in Mill's case, Wiener dreamt up cybernetics. For an account of Wiener's training, see Wiener (1953).

5. von Bertalanffy (1968, pp. 17, 21-22) claims that cybernetics is just a special case of general systems theory.

6. As an example of this view, we can quote R. Theobald:

> Man will no longer need to toil: he must find a new role in the cybernetics era which must emerge from a new goal of self-fulfillment. He can no longer view himself as a superanimal at the center of the physical universe, nor as a super-efficient taker of decisions self-fashioned in the model of the computer. He must now view himself as a truly creative being in the image of a creative God (in Dechert, 1966, p. 69).

7. This spirit is captured in the very etymological roots of Wiener's choice of appellation for his new outlook. "The term 'cybernetics' derives from the Greek word kybernetes, which means steersman. Plato used it to describe the prudential aspect of the art of government" (Dechert, 1966, p. 11).

8. Paradoxically, this seems to have been Wiener's greatest fear.

9. See Dorfman et al. (1958, pp. 1-5) for a short historical sketch of the development of linear programming from a Western perspective. We now know that the same technique was discovered independently, at a much earlier date, by the Russian mathematician Leonid Kantorovich. For a historical account of Kantorovich's work, see Katsenelinboigen (1980, pp. 33-35).

10. The correspondence in Keynes (1973b, pp. 385-406) and his well-known exchange with Tinbergen, in the Economic Journal of 1939-1940 (Keynes' side is reprinted in Keynes, 1973b, pp. 306-320), reveal Keynes' trepidation at endorsing statistical operationalizations of his work.

The relevant passage I refer to in the paper is from the latter exchange:

> No one could be more frank, more painstaking, more free from subjective bias or parti pris than Professor Tinbergen. There is no one, therefore, so far as human qualities go, whom it would be safer to trust with black magic. That there is anyone I would trust with it at the present stage or that this brand of statistical alchemy is ripe to become a branch of science, I am not yet persuaded. But Newton, Boyle, and Locke all played with alchemy. So let him continue (Keynes, 1973b, p. 320).

This is Keynes' polemical skill at its most biting. For a more straightforward account of his views, and a defense of Tinbergen by Roy Harrod, see the correspondence referred to above.

11. See von Bertalanffy (1968, pp. 14-15) for his recollections of the founding of this society and Boulding (1974, p. 67) for his.

12. Leamer (1983) provides a critical view of the usefulness of econometrics from within the ranks. See Fox and Kaul (1980, pp. 4-33) for a review of the multidisciplinary influences on modern econometric work.

13. On this point, Boulding stated that:

> Many and perhaps all of the theoretical systems of economics would qualify as general systems, for they are certainly relevant to other disciplines. The theory of the general equilibrium of prices and outputs of commodities, for instance, as originally developed by Walras, and made operational by Leontief in his input-output analysis in the 1930s, is clearly a special case of a general system of the utmost importance, for it is a special case of the general equations of ecological equilibrium (Boulding, 1974, pp. 67-68).

14. While Walras' classic Elements d'economic politique pure was finally published in 1874, the original debut of his system seems to have taken place a year earlier in a paper presented to the Academie des Sciences Morales et Politiques entitled "Principles of a Mathematical Theory of Exchange." It was completely ignored. See further Walsh and

Gram (1980, pp. 146-147).

15. "We should never forget, however, that Walras' formal schooling had been mainly in the natural sciences and mathematics, and this was available to him when, later, he needed it" (Walsh and Gram, 1980, p. 144).

16. "Walras is interested in the attainment of equilibrium in the market--in the pure theory of exchange" (Walsh and Gram, 1980, p. 147).

17. Consider the following passage form the locus classicus of the modern neo-Walrasian literature:

> To be precise, an economy is defined by m consumers (characterized by their consumption sets and their preferences), by n producers (characterized by their production sets), and the total resources. A state of the economy is a specification of the action of each agent, and a state is said to be attainable if the action of each agent is possible for him and if the action of each agent is possible for him and if their (m+n) actions are compatible with the total resources. The set of attainable states plays an essential role; its properties are therefore studied. A special class of economies is then considered, namely the private ownership economies where consumers own the resources and control the producers. Given a price system, each producer maximizes his profit, which is distributed to consumers-shareholders. The wealths of the latter are thus determined, and they satisfy their preferences under their wealth constraints. As a result of this process, each agent chooses an action. These (m+n) actions are not necessarily compatible with the total resources. Can one find a price system which makes them compatible? An answer is given in section 5.7 in the form of an existence theorem (for which way is prepared by the result of section 5.6). This fundamental theorem of the theory of value explains the prices of all commodities and the actions of all agents in a private ownership economy (Debreu, 1959, p. 74).

While there may be some reason to doubt the sweeping contention of the last sentence, it cannot but be noticed that, as far as the economics goes, Debreu is merely decanting Walras into a more recondite vessel.

18. "However, so far as pure theory is concerned, Walras is in my opinion the greatest of all economists. His system of economic equilibrium, uniting as it does, the quality of 'revolutionary' creativeness with the quality of classic synthesis, is the only work by an economist that will stand comparison with the achievements of theoretical physics. . . . It is the outstanding landmark on the road that economics travels toward the status of a rigorous or exact science and, though outmoded by now, still stands at the back of much of the best theoretical work of our time" (Schumpeter, 1954, p. 822).

19. The remarks of Warlas' translator William Jaffe on pp. 7-9 of the preface to Walras (1954) shed some light on the neglect of the Elements for so long in the English-speaking world.

20. Mark Blaug (1978, p. 617) claims that Value and Capital is "a book which was largely responsible for the revival of Walras in modern economics."

21. "The great economists of earlier generations--Alfred Marshall, A. C. Pigou, and Lord Keynes--thought that mathematical economics had a slim past and no future at all. Time makes fools even of great men. In the middle third of the 20th century, mathematics has everywhere swept through economics like an epidemic of measles sweeping through a new continent" (P. Samuelson in the foreword to Georgescu-Roegen, 1966).

22. "It is important to note that the existence proof shows that at least one equilibrium exists. It does not, and could not, show that only one equilibrium exists. The possibility that there will be multiple equilibrium cannot be excluded" (Bliss, 1975, p. 30).

23. "Someone who demands of a model as complicated as the atemporal equilibrium model what causes a particular value to be what it is should normally expect to be told that everything, meaning all the primitive specifications of the model, have played a part in causing the variable concerned to take whatever value has arisen" (Bliss, 1975, p. 36).

24. For a nontechnical review of the stability literature, see Weintraub (1979), Chapter 7. Arrow and Hahn (1971), Chapters 11-13, provide a complete technical account.

25. When Hicks returned to the stability issue, in his "additional notes" of the 2nd edition of Value and Capital, he bowed to Samuelson's superior mathematical treatment of the stability problem, but retained an interesting skepticism:

> By my hypothesis of essentially instantaneous adjustment, I reduced the purely mechanical part of my dynamic theory to the simplest terms--it is now quite evident that I over-simplified it. But in so doing, I did leave myself free to make some progress with the less mechanical parts--expectations and so on. I still feel that this procedure has its uses, and I should be sorry to abandon it altogether in favor of a pure concentration on mechanism . . . for the understanding of the economic system we need something more, something which does refer back, in the last resort, to the behavior of people and the motives of their conduct (p. 337).

26. "ADM" is the generally accepted adjective for neo-Walrasian theory.

27. Two extreme examples of the powerful role knowledge assumptions play can be seen in the work of the rational expectations theorists, on the side of complete omniscience, and the work of G. L. S. Shackle (1972), on the side of crippling ignorance.

28. He continues: "But this is by no means unconnected with the other question which might be discussed under the same title, the question to what extent formal economic analysis conveys any knowledge about what happens in the real world."

29. To a modern economist this is a surprisingly up-to-date list to which we might want to add macroeconomics and forecasting of all types.

30. "I have long felt that the concept of equilibrium itself and the methods which we employ in pure analysis, have a clear meaning only when confined to the analysis of the action of a single person."

31. Hayek cites Kaldor (1934) in this context, but the description would equally well apply to the rational expectations school.

32. See Burmeister (1980) and Frydman (1982) for some conceptual problems such full knowledge assumptions can create.

33. For an attempt by two modern "Hayekians" to elaborate on the economic role such knowledge institutions play, see O'Driscoll and Rizzo (1985).

34. Bliss (1975, p. 32) makes note of this openness of the neoclassical system:

> Economic theories that attempt to explain prices or distribution from the postulate of a general equilibrium of supply and demand have sometimes been criticized for neglecting the influence of sociological and historical factors as though the "laws of supply and demand" embodied only the working out of purely economic forces. Plainly this is a misunderstanding. If there is a valid explanation of prices and distribution in terms of an equilibrium of supply and demand (always remembering that this may or may not be the correct theory--it is not a matter of logical necessity) then the influence of sociological and historical factors will be chaneled through the excess supply functions of the various actors and in that sense the operation of these influences will be direct. To say that many factors will operate indirectly is not to

devaluate their importance. It is, of course, largely "non-economic" factors that will give to any particular case its particular character.

35. Pears (1970) provides a comprehensive survey of Wittgenstein's thought.

36. Support for this view can be found, among other places in an elliptical reading of Sraffa's review of F. A. Hayek's Prices and Production (Sraffa, 1932a). The content of most of the review consists of Sraffa's attempt to demonstrate logical inconsistency in Hayek's theoretical polemic for a "neutral" monetary policy. But after having spent considerable energy in this pursuit, Sraffa implies that the whole exercise is a misguided one in that the model's premises exclude all of the truly interesting features of a monetary economy from the start. This compares interestingly with Wittgenstein's attitude that his Tractatus work was not wrong, but just misguided in its aim; i.e., that, in fact, the most interesting elements of language were those that his positivistic system was forced to be silent about.

ESSAY II. IN SEARCH OF A MONETARY THEORY OF VALUE: SRAFFA, KEYNES AND THE OWN-RATES THEORY OF INTEREST

I. INTRODUCTION

In 1947, Keynes' biographer, pre-publication critic, and collaborator R. F. Harrod, summed up the General Theory as follows:

> The theory of interest is, I think, the central point in his scheme. He departs from old orthodoxy in holding that the failure of the system to move to a position of full activity is not primarily due to friction, rigidity, immobility or to phenomena essentially connected with the trade cycle. If a certain level of interest is established which is inconsistent with full activity, no flexibility or mobility in the other parts of the system will get the system to move to full activity. But this wrong rate of interest, as we may call it, is not itself a rigidity or inflexibility. It is natural, durable, and in a certain sense, in the free system inevitable. That is why he lays what may seem an undue emphasis on the doctrine that interest is essentially the reward not for saving but for parting with liquidity. Given the complex forces affecting liquidity preference, such and such is the rate of interest that will naturally and necessarily and, so long as underlying forces remain unchanged, permanently obtain. Yet that rate of interest may be inconsistent with the full activity of the system (1947, pp. 69-70, italics added).

Such an extensive quotation is justified by two remarkable qualities of the statement. First, Harrod's statement, when fully digested, can be seen to embody a viewpoint at odds with almost all major conceptions of the meaning of Keynes' theory of employment. As a moment's reflection over the quotation will reveal to the modern economist, this "central point in his scheme" is not compatible with any standard "Keynesian" exposition of underemployment equilibriums. In the "Keynesian" case, the definition of unemployment is its correspondence with some sort of "rigidity" in an otherwise smoothly operating general equilibrium system. Among some old Keynesians, the rigidity was explained as the interest rate becoming stuck in the "liquidity trap" by a pessimism so severe that no amount of inducement could satisfy the public's craving for liquid

balances. When this case was recognized as both theoretically and empirically implausible (see Keynes' own view in Keynes, 1936, p. 207), attention shifted to inflexibility in the labor market. By the logic of the neoclassical synthesis, deviations from full employment had to be due to some non-clearing rigidity in the labor market. It was the final recognition of this point that led to both the breakdown of the Keynesian consensus and the attempt to formulate microfoundation explanations for "rationally" explaining why the labor market might not clear.

More recently, even the most sophisticated attempts to formulate a "Keynesian" theory of unemployment, the French school's "non-price rationing" models (Malinvaud, 1977) and the UCLA school's "non-clearing-market" states (Clower, 1965; Leijonhufvud, 1981), have depended on rigidities to cause the system to deviate from the benchmark case of full Walrasian equilibrium. With the UCLA school, the rigidity is cast in terms of Austrian-styled concerns over incomplete information and a subsequent breakdown of price signals to bring agents' plans into compatibility. For the French school, the point is more technical, but conceptually similar. They want to follow out (in extreme detail) the macroeconomic implications of the lack of market-clearing for a variety of reasons ranging from quantity adjustments (non-price rationing) to fixed nominal prices (fix-price models). Again, the unifying theme in all these cases is that the analytical definition of unemployment states is identified with a deviation from a putative full equilibrium. Thus, only by restricting the system with an exogenously defined rigidity can we theoretically account for unemployment.

Notwithstanding these analyses, Harrod tells us Keynes concluded that "no flexibility or mobility in the other parts of the system will get the system to move to full activity." What might Harrod be driving at with his assertion that Keynes' central point is a "wrong rate of interest," that this rate "is not itself a rigidity or inflexibility," but is "natural, durable and in a certain sense . . . inevitable?" That question is answered by recourse to the second remarkable quality of Harrod's statement, which is that in a concise, shorthand way, he expresses the central concerns of Keynes' own post-General Theory restatements of his revolutionary new theory. In these papers (1937a,b,c), Keynes addresses himself almost entirely to monetary concerns, trying to elaborate his theory of employment via his theory of the interest rate. A variety of different approaches and points are raised in these papers, including Keynes' views on uncertainty, expectations and the role of money and interest in his theoretical explanation of unemployment equilibriums. Taken as a whole, Keynes' post-General Theory defense of his position reveals his feeling that his monetary theory of the interest rate clearly distinguishes him from his predecessors and contemporaries, and was generally being misunderstood by his interpreters. The implication of both Harrod's statement and Keynes' own emphasis on monetary matters in defending his theory, is that a complete understanding of Keynes' theoretical attempt to explain unemployment is fundamentally related to his views on money and interest.

It is the purpose of this essay to try to elucidate that part of Keynes' monetary theory that is implicit in Harrod's statement and

explicit in Keynes' 1937 defense by analyzing a neglected view of Keynes' monetary views, "the own-rates theory of interest."

In keeping with the enduring nature of Keynes as a theoretical dead hand from the past that continues to dominate macroeconomic concerns, this historical pursuit will involve us to some extent in the discussion of Keynes' work implied by modern positions on unemployment theory (including those already mentioned). Specifically, we will offer a simultaneous solution to an historical puzzle and a modern one. The historical question arises from the interpretation of Keynes that forms the basis of the UCLA school mentioned above. It will be the position of this essay that Leijonhufvud's influential view of Keynes as an Austrian-style economist, concerned with the breakdown of the price-signalling functions of interest rates, is in substantial error. A clear contradiction to this view can be found in the very origins of the analysis of Keynes' interest rate theory, the own-rates theory of interest, set forth in Chapter 17 of the General Theory. For the historical origin of the "own-rates" approach to interest rates can be traced to an explicit critique of the Austrian approach to business cycles and monetary theory by Piero Sraffa. Keynes' elaboration of Sraffa's critique into a framework for his analysis of asset holding and money will present a clear alternative view of Keynes' liquidity preference theory from that implied by the Austrian interpretation.

In a more constructive vein, the analysis of the own-rates theory of interest can also be used to elaborate the monetary concerns implied by Harrod's statement and by Keynes' own post-General Theory writings.

Again, there is a counterpart of the historical record in the contemporary literature since the one major school of Keynesian scholarship left out of our discussion so far, the Post Keynesian school (Davidson, 1972; Shackle; 1972; 1974; Kregel, 1973), explicitly ground both their interpretation of Keynes and their future research agenda in the monetary concerns that Keynes raised in 1937. Consequently, our second goal will be to try to show that a substantial agreement with the Post Keynesian view of the importance of Keynes' monetary innovations can be given analytical focus by the "own-rates" approach. The hope is that this view offers an alternative to the sometimes nihilistic attitude towards equilibrium theory implied by the Post Keynesian position on modern developments.

Keynes scholarship is a complicated miasma of incidental historical accounts and controversial analytic debates. This has been the case from the very first "Keynes and the Classics" debate, and it remains so fifty years later. At least partly, this is a legacy of Keynes himself, with his strong appetite for sharp distinctions and keen controversy (1936, p. v). Harrod must have had premonitions of this problem, for after reading drafts of the General Theory, he cautioned Keynes about his theoretical polemics:

> What is important for the initial understanding, which is so much to be desired, is that their minds should be strongly directed on to your essential points. . . . The mind likes to take refuge from the un-familiar (your views which you want to put across) with the familiar (what exactly did Marshall mean in such and such a passage). And I don't think you lead them pleasantly from the familiar to the unfamiliar (Keynes, 1973a, p. 556).

From the standpoint of the evolution of both Keynes scholarship proper, and macroeconomic theory generally, perhaps Keynes was not clear enough in his distinctions between his own and the "classical" theory of the rate of interest. Milgate (1982) has recently put forward this view and laid much of the blame at the feet of Harrod for urging caution on Keynes. But perhaps Harrod understood better than Keynes the extent to which dialogue in economic theory depends on a shared framework of language (equilibrium). What both Harrod and Keynes underestimated, though, was the difficulty involved in altering the orthodox vocabulary. If Harrod is correct that Keynes' view of the central place of the interest rate in his scheme involved a conception of unemployment as "natural, durable and, in a certain sense, in the free system inevitable," then what is required is a reversal of the usual procedure of defining unemployment equilibriums. Thus, for Keynes, as we will try to show, the "natural" rate of interest is not a benchmark case against which the "market rate" is analyzed. And the conception of equilibrium as consistent in its fullest meaning (full equilibrium, full long-period equilibrium, etc.) with full employment is replaced by Keynes with a natural state of unemployment equilibrium of which the state where unemployment is zero is the special case. This is one way of viewing both the Harrod-Keynes correspondence and Keynes' own famous prefatory remarks to the General Theory.

> The ideas which are here expressed so laborously are extremely simple and should be obvious. The difficulty lies, not in the new ideas, but in escaping from the old ones, which ramify, for those brought up as most of us have been, into every corner of our minds.

Before we can get to Keynes' simple and obvious ideas on monetary theory, it is necessary to highlight some of the ideas that ramify into every corner of a modern economist's mind when he thinks of Keynes.

II. THE KEYNES LEGACY: MUDDY WATERS

> There can be no doubt, I think, that Keynes is generally recognized as the predominant figure among economists of this century. Yet I submit that there is still, more than two decades after his death and more than three decades after his General Theory, considerable uncertainty about exactly why he occupies this position (Leijonhufvud, 1968, p. 40).

Almost two more decades have passed since Leijonhufvud and others set out to correct the paradox of the "Keynesian" economists who hardly knew the lineage of their title. Just as he penned these lines, there was beginning an explosion of interest in Keynes scholarship that remains unabated to this day. If nothing else, this mountain of work attests to the endurance of Keynes' paradoxical position as a figure of generally recognized predominance, whose actual ideas are a continuing source of perplexity to the economics profession. In a manner reminiscent of Keynes' own views on expectations of the future, Keynes' important place in economics is secure, but uncertain. Was Keynes a theoretical innovator of the first rank, or just a shrewd polemicist and statesman? Keynes scholarship has still not answered Leijonhufvud's question.

As is often the case with genealogical research, the motivations and results of Keynes scholarship are grounded in a complex combination of idle (professional) interest in the history of ideas, invidious grasping for honorary pedigree, and a committed search for self-knowledge in a past record that may be of current and future use. And, just like the man whose coat of arms must change to accommodate his bank balance, the evolving fortunes of economic theories have influenced the evolving

interpretation of Keynes' work.

Synergism between historical interpretation and theoretical advance is nothing new in economics or peculiar to Keynes scholarship. In fact, it could be argued that this relationship is the most important justification for making a separate discipline of the history of economic thought. Leland Yeager (1973, p. 63) clearly expresses this opinion:

> Cultivation of the history of thought is more necessary in economics than in the natural sciences because earlier discoveries in economics are more in danger of being forgotten; maintaining a cumulative growth of knowledge is more difficult. In the natural sciences, discoveries get embodied not only in further advances in pure knowledge, but also into technology, many of whose users have a profit-and-loss incentive to get things straight. The practitioners of economic technology are largely politicians with rather different motives. . . . In economics, consequently, we need scholars who specialize in keeping us aware of earlier contributions and so enable us to recognize earlier successes--and earlier mistakes--when they surface as supposedly new ideas. By exerting a needed discipline, specialists in the history of thought can contribute to the cumulative character of economics (quoted in O'Driscoll, 1977, p. 1).

Yet, the contemporary state of Keynes' scholarship presents an even more important opportunity than just expressing the "cumulative character" of theoretical development. Since today whole schools of economic theory explicitly base their current work on an historical interpretation of Keynes, Keynes scholarship has exciting (and controversial) contemporary relevance. For this reason, much of the motivation of the recent outpouring of work on Keynes is centered on a dissatisfaction with the current state of economic theory, especially the dominant schools of macroeconomics.

In the period since Leijonhufvud caught (and partly created) this wave of interest in the ideas of Keynes, the fortunes of Keynes'

theoretical legacy, macroeconomics, have been tumultuous. And with each new view on crucial macroeconomic issues comes a new view of Keynes' historical stature. Almost as Leijonhufvud (1968) was writing of the "Keynesian" economists of the neoclassical-synthetic type, this postwar consensus was dissolving. The causes of this "crisis in Keynesian Economics" (Hicks, 1973) are complex enough to warrant full length studies of its own. Some generally recognized elements include: 1) Empirical contradictions of Keynesian theoretical predictions and a seeming lack of policy relevance to the problems of a stagflation era; 2) theoretical challenges from monetarism and its hybrid variant, rational expectations macro theory; and 3) a lack of appeal to a new generation of theorists more highly trained in mathematics--a generation of theorists whose macroeconomic interests, if they had any, ran to the complications of consistently grounding traditional macroeconomic effects in choice-theoretic "microfoundations."

Surely this set of objections to the Keynesian consensus, while by no means complete, is formidable enough to warrant the term "crisis." They go a long way toward explaining the emergence in the 1970s of a general dissatisfaction with a Keynesianism whose only theoretical innovations over the classics seemed to be the empirical <u>assertion</u> of rigid wages. By 1981, the reputation of "Keynesianism" had become the stuff of academic humor:

> There obviously was an American Keynesian school in the days of Alvin Hansen's famous Harvard seminar and for some time thereafter. One cannot off-hand date its demise. But it is doubtful that anyone who has gained prominence in the profession and is now under the age of 40 would accept the label "Keynesian" for himself. So we know

the school is done for (Leijonhufvud, 1981, p. 177).

After the break-up of the Keynesian orthodoxy, the theoretical void in macroeconomics was filled by the rational expectations revolution which seemed to meet objections (2) and (3) above. Not only did the idea that expectations are an economic choice like any other provide new uses for monetarist simplifications of the quantity theory tradition, but it also had the virtue of explicit reliance on maximization hypotheses consistent with traditional choice theory. And, in a manner reminiscent of Yeager's allusion to economic politicians, the policy impotence that was commonly deduced from rational expectations models also provided a solution to objection (1) by denying that employment policy was even possible. In an era where political dissatisfaction with high rates of inflation ran high, the stage was set to try to control inflation with monetarist measures (Peterson, 1985; Kaldor, 1982; Tobin, 1985). The traditional Keynesian objection to such policy became irrelevant under the "Lucas critique," since real employment effects were out of the policy makers' hands anyway.

Although the rational expectations injection into the arm of the monetarist research program has provided enough puzzles that it still dominates macroeconomic research in all of the most prestigious journals, it has never achieved the breadth and strength of the earlier Keynesian consensus. It is still too soon for the ultimate influence of rational expectations theory on the development of macroeconomics to be debated. But the effect on the standard, non-historian's view of Keynes that this "revolution" engendered is clearer. By rational expectations standards,

the Keynesian episode was a deviation (that is gladly over) from the traditional concern of grounding economic behavior in rational choice theory. The rational expectationists show very little interest in the history of their subject, but when they have, they trace themselves to the pre-Keynesian "classical theorists" who looked for the causes of business cycles within the orthodox theory of individual maximization (Lucas, 1981a). By this reckoning, "Keynesian economics is dead" (Lucas, 1980), and for good reason, since "the most rapid progress toward a coherent and useful aggregate economic theory will result from the acceptance of the problem statement as advanced by the business cycle theorists, and not from further attempts to refine the jerry-built structures to which Keynesian Macroeconomics has led us" (Lucas, 1981a, p. 216).

It is interesting to note that the rational expectationists' historical view that the whole Keynesian episode is best forgotten, and that we should go back to take up business cycle theory where the pre-Keynesians left off, has a clear reflection in the current state of macroeconomics. In some ways, the present situation closely resembles the period prior to the Keynesian Revolution. Then, as now, there was an obvious splintering of views on "business cycle theory," with many competing groups contesting the field (Haberler, 1937). Keynes (1936, p. vi) complained of "the deep divergences of opinion which have for the time being almost destroyed the practical influence of economic theory." Today, after a monetarist experiment has been tried and failed to achieve a costless deflation (Kaldor, 1982; Maharidge and Williamson, 1985),

there is no clear guide in economic theory to practical policy (Peterson, 1985; Friedman, 1985). Consequently, many theorists are now searching their theoretical roots seeking answers like religious pilgrims in a time of spiritual crisis. Much of this journey into the desert has involved historical work on Keynes' own writings.

If, by the standards of the rational expectationists, Keynes' place in the history of theory is both securely and certainly minor, it must be emphasized that theirs is a minority view. As already noted, the New Classical School is just one of the reactions to the break-up of the old Keynesianism. Of the other reactions to the disillusion with orthodoxy, two views of macroeconomics, both of which stem explicitly from distinct interpretations of Keynes' own work, are particularly relevant to a discussion of the importance of money and interest in Keynes' theory. Specifically, Leijonhufvud's (1968; 1981) influential reinterpretation of Keynes forms the doctrinal basis for the UCLA approach to macroeconomics as the economics of non-clearing market states; while the views of Shackle (1967; 1972; 1974) and Davidson (1972) form the interpretive basis of the Post Keynesian school.

This contemporary relevance of modern Keynes scholarship can be seen in the seminal work of <u>tertiary</u> literature on Keynes (interpretations of interpretations of Keynes!) by Alan Coddington (1983). Coddington classifies Keynes' scholarship according to the three categories of "Hydraulic Keynesians," "Fundamentalists," and "Reconstituted Reductionists." Hydraulic Keynesians is his term for those proponents of the neoclassical synthesis who once formed the consensus view of

macroeconomics. "This designation reflects the view that the natural and elementary way to regard elementary textbook Keynesianism is as conceiving of the economy at the aggregate level in terms of disembodied and homogeneous flows" (1982, p. 102). As we have seen, believers of this brand of Keynesianism survive today only under deep cover (e.g., in principles courses).

The other two schools of thought on Keynes explicitly arose out of a dissatisfaction with the neoclassical hydraulic Keynesianism. Out of the welter of interpretations of Keynes, Coddington's classificatory schema usefully summarizes two main approaches that are at odds with each other, not only in terms of their view of what was important in Keynes, but also what use should be made of his ideas to guide future research in macroeconomics. For this reason, it will be useful to frame our discussion of Keynes scholarship in terms of his categories (while not necessarily agreeing with Coddington's descriptions and evaluations of these theories). Interestingly for our paper, a major point of difference between the two schools centers around their views of Keynes' monetary theory of the rate of interest. Since both the Fundamentalist and the Reductionist views on Keynes clash with each other over the question of interest rate theory, we will first take a short look at Keynes own post-<u>General Theory</u> views on his interest rate theory. Then we can center our discussion of the modern monetary theories around the issues raised there to set the stage for the own-rates approach to these questions.

III. REDUCTIONISTS, FUNDAMENTALISTS AND KEYNES ON INTEREST AND MONEY

Confusion still swirls about the meaning and validity of Keynes' monetary theory of the interest rate. The writers responsible for the Keynes versus the Classics debate, the New Economics, the Keynesian-Monetarist squabble, the "Economics of Keynes," and now the Post Keynesian School have all argued bitterly over this question, and yet it remains a central mystery. From an historical perspective, we know that this was Keynes' own greatest concern with the reception of his General Theory. Almost his entire published corpus of immediate post-General Theory writings on the book are dominated by monetary concerns. In "The Theory of the Rate of Interest" (1937a), "Alternative Theories of the Rate of Interest" (1937b), and "The General Theory of Employment" (1937c), Keynes tried a variety of tacks to distinguish his monetary theory from that of his predecessors and contemporaries. From all of these and his private correspondence of the period, we get the impression that he felt that this part of his book was both particularly important and particularly misunderstood.

A brief look at these articles will set the stage for our discussion of both the modern views on liquidity preference theory and the development of the own-rates theory itself.

It is convenient to start our discussion of Keynes' defense and development of his interest rate theory with an article he contributed to a festschrift for Irving Fisher in 1937. "The Theory of the Rate of

Interest" is relevant to our theme in that it is explicitly concerned with differentiating Keynes' interest rate theory from "orthodox theory," and it explicitly uses the own-rates framework towards this goal. Keynes begins his discussion by outlining his view of the content of this orthodoxy in the form of four propositions (1937a, pp. 101-103).

(1) Interest on money is simply "the premium on current cash over deferred cash." People pay this premium because cash has some service yield. Thus, "we can conveniently say that interest on money measures the marginal efficiency of money measured in terms of itself as a unit."

(2) "Money is not peculiar in having a marginal efficiency measured in terms of itself." So do stocks of commodities and all other assets. These can be either positive or negative and can be derived from the relationship between spot and future prices of the asset.

(3) Arbitrage by wealth holders will lead to an equality of marginal efficiencies of all assets when measured in a common unit--money, for instance.

(4) If the demand price of a capital asset determined on the secondhand asset market yields a marginal efficiency in terms of money that is higher than the money rate, that asset will be newly produced (i.e., investment will flow towards its production). The scale of this investment for any asset is determined by its cost of production and the rate at which the profitability of the investment declines as it goes forward. "Thus the price system resulting from the relationships between marginal efficiencies of different capital assets including money, measured in terms of a common unit, determines the aggregate rate of

investment."

Keynes claims that these four propositions, which we will see are cast more in terms of his own system than of the orthodoxy, are "consistent" with orthodox theory in showing how the system of relative prices in conjunction with the scale of output move until the marginal efficiencies of all kinds of assets are equal, when measured in a common unit. Utilizing his own peculiar language, Keynes is really just stating the orthodox conception of a long period equilibrium where rates of return (marginal efficiencies) are equal in all sectors. He speculates that this conception is the source of the idea that the interest rate is determined by productivity considerations, since in equilibrium the rate of money interest equals the marginal efficiency of capital goods.

"But they [the 4 propositions] tell us nothing as to the forces which determine what this common level of marginal efficiency will tend to be. It is when we proceed to this further discussion that my argument diverges from the orthodox argument" (p. 103). According to Keynes, the uniqueness of his theory rests with the contention that the forces determining the scale of the equilibrium position are not independent of money, but that in the usual case just the opposite is true:

> . . . namely that the marginal efficiency of money is determined by forces partly appropriate to itself, and that prices move until the marginal efficiencies of other assets fall into line with the rate of interest (p. 103).

He suggests that the orthodox position implicitly requires two further propositions, and that these are the points on which they differ. In order to substantiate the orthodox emphasis on savings and investment

as the ultimately determining factors of the rate of interest, it is necessary to assume these further points:

(5) "The marginal efficiency of money in terms of itself has the peculiarity that it is independent of its quantity. In this respect, it differs from other capital assets." Keynes attributes this proposition to "the quantity theory strictly stated." By this, he means that in the equation of exchange, any quantity of money can satisfy the demand for money, implied by, say, the Cambridge k. He further claims that since any quantity of M is consistent with the equation, that in orthodox theory the rate of interest on money is itself indeterminate. Thus follows the normal procedure of defining the money rate by its necessary equality with the marginal efficiency of capital assets.

(6) But since, by this account, the rate of interest is not determined until we know the scale of investment, "the scale of investment will not reach its equilibrium level until the point is reached at which the elasticity of supply of output as a whole has fallen to zero" (p. 104). This is Keynes' preferred post-General Theory definition of full employment where no more aggregate demand can call forth any new production.[1] Here, Keynes is driving at the point that the full employment of resources is the only equilibrium level of output if it is the supply of productive factors (savings) and their productivity (the marginal efficiency of capital) that determines the rate of interest.

According to Keynes, the addition of those two propositions to (1)-(4) is the only way that the orthodox system can be made logically

consistent in terms of its interest rate theory and monetary theory. The "final synthesis of this theory," then, is that

> . . . the position of equilibrium is given by that common value of the rate of interest and of the marginal efficiency of capital at which saving determined by the former is equal to the investment determined by the latter (p. 104).

Accordingly, it is by restating these implicit assumptions of the classical analysis that Keynes delineates his approach and parts company with his predecessors. For (5) and (6), he would substitute:

(5*) "The marginal efficiency of money in terms of itself is, in general, a function of its quantity (though not its quantity alone), just as in the case of other capital assets." So money is just another capital-asset from this standpoint.

(6*) "Aggregate investment may reach its equilibrium rate under proposition (4) above, before the elasticity of supply of output as a whole has fallen to zero" (p. 104). The interest rate in his theory can equilibrate at any level of employment.

This article provides a nicely schematized version of Keynes' own position on his relation to orthodox theory. The framework chosen to characterize the arguments is the own-rates theory of chapter 17 that we treat in detail in section V. Its use here is pertinent to our theme in a number of ways. First, it emphasizes the generality of the own-rates approach (it can express the orthodox theory as well as Keynes' own). Also, we get a glimpse here of our interest in the monetary terms upon which Keynes claimed generality for his own theory (it applies to interest rates at all levels of employment, not just the full employment

case). We will see that in chapter 17 Keynes uses this last general quality to subsume the Wicksellian "natural rate" as a special case of his general theory of interest. Finally, Keynes' use of the own-rates equilibrium framework (which we have yet to specify) to reemphasize his departure from the orthodox theory of interest adds weight to our hope that it will serve a useful role as a means of understanding the special features of liquidity preference theory.

A further point brought out in this admittedly brief discussion (it was meant as a defense of the book he had already written and so assumes a familiarity with it) is that the framework of analysis runs in terms of an integrated view of monetary, value and production theory. As we will see when we get to our detailed analysis of the own-rates theory, it was a major preoccupation of Keynes that his monetary theory should be fully integrated with the rest of his scheme. Accordingly, he analyzes monetary concerns as integral to both the allocative flows of resources into different uses (investment above) and to the determination of the whole system of relative prices.

The linkages between these theoretical domains can already be discerned in this brief treatment. Wealth owners and arbitragers trade existing assets (including money) until expected rates of return (implied by spot and future prices) are equal for all existing assets. If this level of asset market equilibrium sets a price for a capital good (investment) that is higher than its replacement cost, it will be profitable to newly produce that good. As this investment goes forward, rates of return fall as profitable production of each production process

reaches the limits of the market, given existing cost and demand conditions. Thus, relative prices, the level of resource utilization, and interest rates are all determined together. Money as an asset in this framework competes with other forms in which to hold wealth and so enters as an operative factor from the beginning. The difference between (5) and (6), and (5*) and (6*) illustrates this last point. Keynes staked his unique view of how this interrelated system moves toward equilibrium on the possible influence money, held as an asset, might have. In the orthodox theory, money was a "neutral" factor and thus could not hold up the production process. For Keynes, it was an operative factor in the situation which had "real," not just monetary, effects. This will be the central conclusion of the own-rates analysis.

In the festschrift article, Keynes ascribes the basis of his departure from orthodoxy, implied by (5*) and (6*) above, to the special features of his liquidity preference theory. In particular, he emphasizes his views on uncertainty, expectations and the functions of money (1937a, pp. 105-108). But a more complete discussion of these topics is gained from Keynes' second post-General Theory writing, "The General Theory of Employment" in the Q.J.E. for 1937. For our purposes, it is especially interesting to note that in this paper Keynes professes to be interested in trying to reexpress "the comparatively simple fundamental ideas which underlie my theory" (1937b, p. 111), and that in the process of doing so, he spends all of his space discussing monetary matters. For this reason, this paper is often taken to be central to the modern Post Keynesian emphasis on the importance of Keynes' views on

uncertainty and money in assessing his contribution (Coddington, 1983, p. 94).

In the Q.J.E. article, Keynes emphasizes these themes of uncertainty and the role of money to delineate his departure from orthodoxy implied by the festschrift contribution. He particularly takes pains to insist that the world he is describing is one of "uncalculable uncertainty" about the future. By this term, he distinguishes his view from the "Benthamite" idea that we can attach probabilities to future events. "About these matters there is no scientific basis on which to form any calculable probability whatsoever. We simply do not know" (1937b, p. 114). Since we do not have any information on which to probablistically predict this uncertain future, we fall back instead on rules of thumb such as our best guess of the existing state of opinion. This leads to a psychology of conventional judgment.

Keynes stresses that the major impact the recognition of this uncertainty has on economic theory concerns the questions of money, capital and how unspent income will be held. Both in terms of the expectation of capital yields and the reasons for asset preferences, this uncertainty radically alters the view of money in economic theory. In an uncertain world, money takes on special significance as a liquid store of value, and it is to this function that monetary theory should look for the explanation of the interest rate and business fluctuations.

The measure of our doubt ("disquietude") about the future is Keynes' impressionistic characterization of the money rate of interest. "Because, partly on reasonable and partly on instinctive grounds, our

desire to hold money as a store of wealth is a barometer of the degree of our distrust of our own calculations and conventions concerning the future" (1937c, p. 116). Thus, just as the arithmetic books tell us, the rate of interest is the price we require to part with our money or "the measure of the degree of our disquietude." We can interpret these concerns as the foundation for the festschrift's insistence on the unique characteristics of the money rate as opposed to other marginal efficiencies of assets.

The tie-in with the equilibrium framework of the previous paper is made by the assertion that, although money has unique characteristics, it is also just another store of value or asset. Just like money, all other assets exhibit a rate of return that when measured in a common unit must all equal each other. It is here that Keynes reconnects the monetary and real forces which are so clearly separated in orthodox theory. It is the rate of return on capital assets, tied in equilibrium to the rate of return on money, which determines the aggregate rate of investment. To the extent that the unique characteristics of the money rate in an uncertain world affect the equilibrium values of all rates of return, money has "real" effects. "It is not surprising that the volume of investment, thus determined, should fluctuate widely from time to time. For it depends on two sets of judgments about the future, neither of which rests on an adequate or secure foundation--on the propensity to hoard and on opinions of the future yield of capital assets" (p. 118). The argument is completed by tying this fluctuating flow of investment to the "supply and demand for output as a whole." "The theory can be summed

up by saying that, given the psychology of the public, the level of output and employment as a whole depends on the amount of investment" (p. 121).

Finally, in the last paper cited, "Alternative Theories of the Rate of Interest" (1937c), Keynes takes issue with a number of interpretations of his book. Citing B. Ohlin, R. G. Hawtrey, D. H. Robertson, J. R. Hicks and others, he claims that these authors' views of his interest rate theory represent

> . . . a concealed difference of opinion, which is of very great importance, between myself and a group of economists who represent themselves as agreeing with me in abandoning the theory that the rate of interest is (in Professor Ohlin's words) "determined by the condition that it equalizes the supply of and demand for savings, or, in other words, equalizes savings and investment" (1937c, pp. 201-202).

The "concealed difference of opinion," it turns out, centers around the contention by these authors that the interest rate can be represented by a simultaneous solution of a general equilibrium where the supply and demand for money is considered the obverse of the supply and demand for "credit" or "loans." This seems to be the origin of the infamous loanable funds versus liquidity preference controversy. As opposed to the solution generally accepted to that battle (Tsaing, 1966), that the two approaches are equal in equilibrium configurations, Keynes is adamant that the demand and supply of loans is not the same thing as the demand and supply for money. Significantly for us, he bases his claim on the own-rates doctrine. Keynes claims that the general equilibrium conception where the prices of money and credit are indistinguishable

> . . . is altogether remote from my contention that the rate of interest (as we call it for short) is, strictly speaking, a monetary phenomenon in the special sense that it is the own rate of interest of money itself, i.e. that it equalizes the advantages of holding actual cash and a deferred claim on cash (p. 206).

What Keynes is again driving at in this passage is his crucial argument that, in an uncertain world, money is both just another asset like any other in that it trades in terms of a marginal efficiency in terms of itself, and that money has special characteristics which make it an independent factor in the final equilibrium situation. These will be the central questions of the own-rates analysis of chapter 17. Furthermore, to Keynes, the conception of supply and demand for credit used by these authors reduces in the end to saying that the rate of interest equilibrates savings and investment. This is the very classical notion that Keynes is opposed to and that these authors have said they differ with. In Keynes' own system, this conception can make no sense since saving and investment are always equal and fluctuate with the level of income "whatever interest level exists on the market" (p. 206).

The rest of this article leads off into a discussion of the "finance motive" for holding cash which is an interesting topic in itself, but strays from our central concern (see Asimakopulos, 1983; Snippe, 1985). To conclude our review of Keynes' published reactions to the reception of his General Theory in 1936, it is sufficient to reiterate that his central concern was that his interest rate theory was being misunderstood; that the key to his approach lay in the role of money as a unique asset in an uncertain world. By Keynes' account, the role of the interest rate is that of a "monetary phenomenon" with "real" effects.

Our analysis of the own-rates theory will more fully explain Keynes' unique position on monetary matters. But before we get to the own-rates, it will be instructive to look at the description Keynes' interest rate theory has received in the postwar literature.

The above account of Keynes' views on monetary theory is clearly not the usual view of the meaning of the liquidity preference theory of interest. It is surely a far cry from the interest rate theory embodied in the standard "IS-LM" (Hydraulic) Keynesian model. In this static world, the richness of Keynes' vision is reduced to an elasticity property of a narrowly defined money demand function, which equilibrates the demand for a given stock of money with a flow of aggregate investment. In the IS-LM world, the money interest rate can hardly be "wrong"; it is just one determining factor of the composition of output between investment and consumption goods. Any alteration of the "supply of output as a whole," to use Keynes' term, is wholly dependent upon some rigidity in the interdependent Walrasian system. To the extent that Keynesians admitted the improbability of the liquidity trap, the whole weight of involuntary unemployment in this model came to rest upon an ad hoc rigid money-wage (Weintraub, 1979, pp. 66-67).

Monetary specialists, recognizing the importance Keynes attached to his monetary theory, also developed a large literature trying to fit his views on these topics into the static framework of the neoclassical synthesis. As R. W. Clower indicates, much of postwar monetary theory has been devoted to understanding just exactly how we are to interpret this vision that Keynes claimed was a revolutionary new way to look at a

monetary economy:

> Keynes' General Theory of Employment, Interest and Money is, par excellence, a theory of the actual working of a money economy. Unfortunately, Keynes expressed his ideas in language and relations that too easily lend themselves to interpretation within the formal framework of neo-classical equilibrium analysis. Partly for this reason, partly because the actual working of a monetary economy is inherently difficult to portray analytically, economic theorists are still arguing about the precise nature of the so-called Keynesian revolution or, as some would put it, the precise difference between a money and a barter economy. There is a vast literature on this subject, most of it having some bearing on monetary theory. . . " (Clower, 1969, p. 213).

In these more specialized Keynesian explanations of the "liquidity preference theory of the rate of interest" (Tobin, 1958), the role of money is not much closer to Keynes' vision than the IS-LM framework. In the standard portfolio approach explanation, money demand is tied to a preference for a riskless asset that yields no return to balance out the risk associated with more profitable investments. Tobin ingeniously expanded this simple idea of portfolio selection behavior into an inverse relationship between the rate of return on risky assets and the demand for riskless money to offset the total risk to the risk averse investor.

But from the standpoint of Keynes' own concerns, an important element is left out of this story. Most importantly, the uncertain world that Keynes described (1936, p. 168) as the basis of interest on money is translated in Tobin's world into one of calculable risk described by a known probability distribution. In fact, the proper shape of the utility surfaces that give rise to Tobin's liquidity preference depends critically on some very restrictive descriptions of the asset selection decision. Either the utility functions must take on a very specific

functional form, or the outcomes of the different investment decisions must be normally distributed. As Paul Davidson (1972, p. 207) points out in a critique of this view of liquidity preference:

> True uncertainty has an essential characteristic which violates the mathematical laws of probability, namely that there is doubt or disbelief about any possible outcome and therefore, uncertainty about some possible alternative outcomes cannot be reduced to a quantitative fraction whose magnitude is between zero and unity.[2]

Having seen Keynes' own view of his departure from orthodox interest rate theory, we are led to ask what became of uncertainty, expectations and conventional judgments in the Keynesian version of liquidity preference theory. Abstracting from uncertainty as he described it, Keynes asked, "Why should anyone outside a lunatic asylum wish to use money as a store of wealth?" (1937b, p. 116). Perhaps he should have asked, "Outside of a lunatic asylum or a Benthamite world of calculable uncertainty." Noting the narrowness of the framework into which Keynes' views were squeezed by neoclassical monetary theory, we should not be surprised that the protracted Keynesian-Monetarist debate should have been so inconclusive and sterile of further avenues of monetary insights. If we take Keynes' own view of his differences with the classical theory seriously, both the Keynesians and the Monetarists, in ignoring the uncertain basis that Keynes made conditional for his view of monetary theory, were arguing over a small elasticity property of the same classical model.

As for historical work proper, it has been a major occupation of many Keynes scholars to try to explain his monetary and interest rate theory as something different from an assumed functional form of a money

demand equation as it came to be defined in the Keynesian-Monetarist debate. A number of scholars have focused on the monetary aspects of Keynes' work as the embodiment of its revolutionary character. Prominent in this branch of Keynes' scholarship are Paul Davidson, G. L. S. Shackle and Axel Leijonhufvud.

In the classificatory scheme of interpretations of Keynes put forward by Alan Coddington in "Keynesian Economics: the Search for First Principles" (1983), Leijonhufvud is classified along with R. W. Clower as major proponents of the view he calls "Reconstituted Reductionism." By this label, Coddington refers to the position that Keynes' theoretical contribution falls outside the realm of Walrasian tatonnement equilibrium states. In this view, following out the "Economics of Keynes" requires a reworking of the traditional choice-theoretic microfoundations of general equilibrium theory to explain unemployment "disequilibriums." Clowers' (1965) famous distinction between "notional" demands appropriate to a Walrasian situation and "effective" demands that can be constrained by the imbalances of an unemployment disequilibrium were the original attempt to provide these microfoundations. Following Clower (1965) and Barro and Grossman (1971), this conception has led to an enormous literature on complicated general <u>disequilibrium</u> models that identify themselves as Keynesian by virtue of their exhibiting unemployment states. (Though, note they are unemployment <u>disequilibrium</u> states, not unemployment <u>equilibriums</u>. See Weintraub (1979) and Casson (1981) for a survey of these models.)

The other strand of the reconstituted reductionist program is the

concern with the economic costs and uses of information, and the impact of differential information on individual choices, especially in the labor market. By the reasoning of this literature, unemployment can be explained by rational decisions on the part of employees to value each marginal loss from time spent without a job against the expected marginal gain of looking further for a better job. All unemployment thus becomes voluntary to some extent and reflects the rational behavior of agents optimally using the information they can acquire (Alchian, 1970).

The central organizing theme of reconstituted reductionism is its rejection of the tatonnement process by which Walrasian equilibriums are established. When this admittedly inadequate (see Essay I) view of the process of "getting into equilibrium" is rejected, it becomes necessary to search for some other way of describing how individual economic plans come into macroeconomic coordination with each other. As Coddington's term "reductionist" implies, the major focus of the Leijonhufvud view of macroeconomics concerns the individual microeconomic foundations of this coordinating activity. Unemployment, on this view, is by definition a disequilibrium divergence from the full coordination implied by the Walrasian tatonnement process. In keeping with this theme, money serves as a link between individual plans, which by interfacing all transactions (Clower's famous "goods trade against money, not against other goods") can allow economic actors to postpone transactions--usually to gain more information. Thus, because of the availability of liquid holdings, there can arise <u>discoordination</u> (read disequilibrium in Walrasian terms), both intersectorally and intertemporally. Thus, discoordination of the

various individual plans that make up the system is the essence of the unemployment states reductionists are interested in:

> The novel theoretical idea in Keynes's work that was lost sight of in all of this was different. To appreciate--and to appreciate how difficult it is to do it justice within the framework of equilibrium models--one has to envisage the possibility of coordination of "desired" transactor activities in a system failing because communication between them fails to convey the needed information (Leijonhufvud, 1981, p. 332).

As we mentioned earlier, there is a strong link between this disequilibrium outlook on modern macroeconomic concerns and a compatible historical interpretation of Keynes' own work. For the UCLA School, the historical basis of its modern theoretical outlook is the influential work of Axel Leijonhufvud (1968, 1981). In broad outlines, Leijonhufvud's "Economics of Keynes" follows the pattern of reconstituted reductionism just touched upon. It frames Keynes' contribution as providing an alternative vision of the coordination (or discoordination) of economic activity. Strongly influenced by Robert Clower, Leijonhufvud (1968, pp. vii-viii; 1981, p. vii) brings the preconceptions of the reductionist program to his historical work on Keynes. It is the position of this essay that these preconceptions led Leijonhuvud's "Economics of Keynes" away from Keynes' own concerns, especially with regard to his theory of the interest. In particular, we will argue that by starting from a basically Austrian vision of the economic process, the reductionist view is led inexorably to abandon Keynes' liquidity preference theory of interest, since that theory was founded upon a critique of the Austrian monetary approach. If our contention that Keynes' views on interest and money are integral to understanding his

whole system is correct, then this abandonment is substantially an abandonment of Keynes' economics. And our further and more important point is that these views are valuable in themselves in understanding the role of money and interest.

Leijonhufvud's work goes a long way toward refocusing discussion of Keynes away from the mechanical "hydraulicism" of the IS-LM model and back toward some of the issues that Keynes actually dealt with. Yet, his interpretation of Keynes is led astray by its attempt to reinterpret Keynes within an Austrian vision of the economic process. By Austrian vision is meant the combination of concerns that mark the whole reductionist program: attention away from aggregative analysis toward individual decision making, the emphasis on the information aspects of the price system and the consequent methodological preference for individualist, disequilibrium process analysis over the specification of equilibrium states (Kirzner, 1973, ch. 1; O'Driscoll, 1977, ch. 2).

The fact that Leijonhufvud's preconceptions about economic theory are substantially an Austrian view is not hard to establish, nor should it be controversial in and of itself. In his original study of Keynes (Leijonhufvud, 1968, pp. 70, 271, 376), he clearly footnotes his view of the coordination problem of transmitting economic information to the pioneering work in this area of the premier Austrian theorist of this century, F. A. Hayek. Moreover, he ends that study of Keynes with a methodological discussion of his "fresh view" of macroeconomics in which he intends to "indicate the theoretical direction in which I feel the <u>General Theory</u> pointed, and the promise which the Keynesian tradition has

not fulfilled" (p. 389). In this context, he candidly admits his interest in the Austrian approach in taking the "first step" beyond (by his interpretation) Keynes' abandonment of the sterile equilibrium framework:

> But even for such a first step, the prescription cannot be to "go back to Keynes." If one must retrace some steps of past developments in order to get on the right track--and that is probably advisable--my own preference is to go back to Hayek (Leijonhufvud, 1968, p. 401).

Not surprisingly, then, Leijonhufvud's more recent work has led him to elaborate a position on interest rates and the business cycle that is grounded in the Austrian-Wicksellian conception of the "natural rate" doctrine. In his "The Wicksell Connection: Variations on a Theme" (1981, pp. 131-202), we get a clear view of the fact that following out the Austrian theory of the business-cycle inevitably leads to an abandonment of Keynes' views on money and interest. And, as we noted, to the extent that these monetary views are an essential part of Keynes' vision, this involves an abandonment of Keynes' own message and so reveals the false moorings of the "Reconstituted Reductionist" interpretation of Keynes. The full working out of this tension will be implicit in our whole discussion of the own-rates theory of interest. Likewise, the more vital question of the intrinsic value of Keynes' views will necessarily have to wait until we have delved more deeply into them. At this point, I merely want to illustrate that the reductionist view involves the rejection of liquidity preference theory.

Leijonhufvud makes this rejection clear in his attempt to show that any significant advance in macroeconomics must stem from a view of Keynes

within the Wicksellian framework of business cycle theory. Starting from the premise that "the theory of the interest rate is the center of the confusion in modern macroeconomics" (1981, p. 131), he laments the loss of Wicksell's insights into this subject. These insights are identified as stemming from "the discrepancy between the market rate and the natural rate." The loss of this insight is attributed to none other than Keynes:

> What happened, essentially, was that Keynes so obfuscated the interest rate mechanism that the later Keynesian literature almost entirely lost track of Wicksell's theme (p. 134).

As a method of recapturing Wicksell's insight into monetary theory, Leijonhufvud proposes a hybrid of Keynes and Wicksell which he dubs the "Z theory." This "analytical interpolation between the Treatise and the General Theory" is described "either as 'the Treatise plus quantity-adjustments' or as 'the General Theory minus the Liquidity Preference theory of interest'" (p. 134). In either case, the crucial thing is to abandon Keynes' most developed views on interest and money:

> It is my own position that Z-theory incorporates all of Keynes' contribution that should be preserved and developed; that the LP hypothesis should have been rejected from the start; and that, failing this, propositions derivative from it ought systematically, if belatedly, to be rooted out of modern macroeconomics.

Thus, Leijonhufvud calls for rejecting all of the themes Keynes himself stressed in his post-General Theory defense of his views, and he does so to advance Keynesian ideas! It is notable in this connection that the reductionist program outlined above had little to say about money, uncertainty, expectations or interest rate theory. In fact, since the reductionists start their investigation from a benchmark "full information economy" (Leijonhufvud, 1981, pp. 136-140),[3] against which

the "disequilibrium" unemployment states are defined, it can be argued that they are still trapped within the confines of the orthodoxy that Keynes was trying to break out of. There is a necessary corollary here between the use of "information" by the reductionists and "uncertainty" by Keynes on the one hand, and Wicksellian interest rate theory and the liquidity preference theory on the other. When the future is uncertain, in Keynes' sense of unknowable, no amount of information, no matter how well used, can reduce the condition. This distinction becomes especially crucial in the context of interest rate theory since, as we have seen, the foundation of Keynes' views on liquidity preference is the existence of such uncertainty. Sidney Weintraub (1982) makes just this point in a review of Leijonhufvud (1981) where he asks:

> Does AL [Leijonhufvud] mean "information" on present or current moment opportunities, or future "opportunities"? The present, as an instant of time, can never be very important in anything but the most static analysis. It must be future moments, adding up to a stretch of future time. How then talk of "information failures" when the vital data are unobtainable, or impossible to ascertain, until the tomorrows become yesterdays? Surely there is no possibility of "coordinating information" on events that are unknowable because they have not happened. Even an omniscient auctioneer, who knew all the yesterdays, would have to possess the added quality of a seer to know the tomorrows (1982, p. 421).

To see the importance of this distinction for the choice of macroeconomic method, recall that, for the reductionists, unemployment reflects discoordination of individual plans (which include those of saving and investing especially). These maladjustments are conceived of as the consequence of rational optimizing by individuals in response to available information. Since this information may be wrong (because of bank authority distortions, say) or incomplete (because it is too

costly), such optimizing generates deviations from the putative "full information" equilibrium. Thus, a system where the cause of fluctuations in activity is looked for in intertemporal "information failures" (as in the Wicksellian-Austrian tradition of Mises, 1953; Hayek, 1931; 1941; and O'Driscoll, 1977) must rely on the interest rate to provide the (wrong) signals to savers and investors so that their intertemporal plans become discoordinated in the first place. As we saw in Keynes' discussion of his interpreters (1937b), he was adamant that his interest rate theory could not be interpreted as bringing savings and investment into equilibrium, which are always equal by definition anyway and which move with the level of income. Consequently, it is not surprising that an Austrian interpretation of Keynes would eventually run head-on into his views on interest and money. Such a collision is exactly what we see in Leijonhufvud's interpretation of Keynes.

Whether the avenue of advance for macroeconomics is best started from Keynes' own monetary concerns or Hayek's natural rate tradition is an issue that awaits us. What is clearer is that to use Austrian glasses to view Keynes' theoretical contribution (especially his monetary views) must lead to a distorted view of his meaning. For the ultimate development of Keynes' broadest view of monetary theory, that incorporated in his chapter 17 discussion of "The Essential Properties of Interest and Money," stems explicitly from a dissent from the then conventional "natural-rate" idea that dominates the Austrian tradition. It will be one of our central concerns to trace the development of that dissenting view from Sraffa's critique in 1932 through the <u>General</u>

Theory. But before we leave the contemporary literature, it is necessary to emphasize that there is another modern school of thought which traces itself to Keynes that is much more concerned with his views on money and interest, viz., the self-proclaimed Post Keynesian economists such as Davidson, Shackle and Minsky.

Coddington refers to the Post Keynesians as the "Fundamentalists." By dubbing them so, he means to highlight their position that Keynes' views on uncertainty and money constitute a fundamental "frontal assault on the whole reductionist programme." According to this aspect of classification, the Post Keynesians join the reductionists in rejecting the usefulness of Walrasian equilibriums to address Keynes' concerns. But the fundamentalists see this rejection not as a call to reformulate the microfoundations of the Walrasian conception, but as a call to abandon choice theory (as traditionally defined) altogether.

> As against the clearly specified and stable objectives and constraints required by reductionist theorizing, Keynes emphasizes the basis of choice in vague, uncertain and shifting expectations of future events and circumstances. . . . Keynes focused on the conventional element in valuation: the way in which valuations may persist to the extent that they are shared, and are thereby rendered sustainable in the face of minor events and changes in circumstances, but also vulnerable to anything that threatens this conventional basis (Coddington, 1983, p. 95).

These concerns are clearly derived from Keynes' own discussion of his monetary theory that we surveyed above. The Post Keynesians have been adamant in their belief that the Keynesian themes of uncertainty, expectations and the social role of money are so fundamentally opposed to the orthodox method of grounding economic behavior in deterministic individual choice that a whole new approach to theory is called for. For

the most part, this has led to an explicit rejection on their part of equilibrium analysis altogether, as being incompatible with their emphasis on the influences of real time (versus "logical time") and history in any particular economic configuration (Robinson, 1953-54; 1981; Shackle, 1972). By this distinction, the Post Keynesians separate their concerns with the way time, in the sense of expectations of the future, is wholly different from the time of economic equilibriums where the long and short runs are analytically (logically) defined. Jan Kregel (1973, pp. 31-32), a prominent Post Keynesian scholar, clearly explains the importance of the distinction:

> The specification of the equilibrium in the neoclassical framework is broadly similar to that found in the natural sciences. The proof is carried out by showing that equilibrium exists. The economic application then goes on to show that at any point outside of the equilibrium position there are forces that exist to drive the system back to equilibrium. This says nothing about how the system gets to equilibrium in the first place, but proves that such a position exists and that any state of existence outside equilibrium is illogical. . . . Thus the neoclassical belief that the system, left alone for a sufficient period of time, will naturally attain the position of equilibrium at full employment. . . . Equilibrium is the only logically admissable position, historical time is not allowed to exist.

It can hardly be denied that the strict formalism of naive equilibrium theory leads to a neglect of the historical and social particulars of the situation. (We discussed this in detail in essay I.) The problem arises when, having recognized this sterility, we look for a way out. In the past, such attempts have been made by the specialized studies of the German Historical School, the evolutionary approach of the American Institutionalists, and today by both the Post Keynesians and the neo-Austrian schools. The floundering of those past attempts, though,

has always involved the lack of a ready framework of analysis by which a tradition and body of scholarship could be easily passed on and incrementally advanced. When Thorstein Veblen turned his genius to institutional studies, the results were dazzling. Yet, with slight exceptions, his approach has rarely been carried forward with the same success by his followers. One explanation that goes a long way toward understanding of the peculiar staying power of the basic framework of neoclassical analysis, is that its strong foundation of individual choice theory is an infinitely applicable analytical engine. In the face of numerous attacks on both its base and superstructure (for outdated preconceptions, ideological apologism, and empirical irrelevance), the economics of microfounded equilibriums marches on. Now, to a lot (most?) of practitioners, this is a sign that the framework holds water, even if it needs to be stretched in certain places . This is surely the reasoning behind the remarks, quoted above, from the New Classical Economists to the effect that the Keynesian detour is a dead end best forgotten.

Yet, some of us are left wondering. If the implications of an uncertain future are plausibly enough confirmed by our experience that a rational expectation seems quite ridiculous; if the elements of social psychology and conventional judgment that we observe daily in the financial news are <u>logically</u> excluded by a reductionist framework; and, most importantly, if we feel certain that the state of unemployment and not full employment is the normal case, the exception to which needs to be demonstrated, not vice versa, then should not some alternative to the orthodox framework be in order?

It is precisely these issues which the Post Keynesians, building on Keynes, draw our attention to. But, as we argued earlier, the extent to which they have been heard or used seems to rest almost exclusively on the language in which the argument is made. Harrod's statement about Keynes' fundamental points being buried in the backlash from his critical polemics has been partly vindicated by events. It has only been when unemployment is viewed as a deviation from an otherwise smoothly operating classical system that theory has recognized the reality of unemployment. In the face of such an all-engulfing theoretical cooptation, Keynesians have naturally sought other less consuming theoretical frameworks (disequilibrium, historical, etc.). Yet, in doing so, they have suffered the same fate that the institutionalists, the economic historians and other assorted sub-disciplines have. Their interests have been recognized, circumscribed, and declared to fall outside of the realm of equilibrium theory proper. Thus, safely inoculated against each new attack, the core of choice theory can go on its own separate way. The surest sign of this lack of receptivity by the mainstream of the profession is the establishment of a separate journal to air the subdiscipline's work and view. In the case of the Post Keynesians, we have had this situation since 1978.

Consequent to this theoretical isolation, it seems reasonable to try to recast the unique perspective Keynes held on matters of money and interest in a more digestible form. The task is to set up some type of "equilibrium" or centering device that does not, by its very nature, exclude the role of historical time and uncertainty on the economic

configuration. This very goal seems to be implied by the latest controversies in Cambridge economic theory (Milgate, 1982; Eatwell and Milgate, 1983; Kregel, 1985) which have centered on the choice of an alternative framework to the Walrasian general equilibrium paradigm. In this literature, the views of Garegnani (1976; 1978; 1979) and Milgate (1982) represent the attempt to ground economic theory in the long period value theory of the classical economists, along the lines of the classical revival laid down by Sraffa (1960). Starting from the belief that the capital theory controversies of the last decades (Harcourt, 1972) have discredited neoclassical theory, the Cambridge school is attempting to "go back to (one view of) the Classicals" for an untainted framework. An important element of this work concerns the way that Keynes fits into such a picture. To Milgate and Garegnani, the task is to strip Keynes of his marginalist methods and graft the residual aggregate demand analysis onto the Sraffa price system (see Milgate, 1982). Not wanting to get into the complications this literature involves, we wish only to use it to illustrate two points. First, this literature explicitly addresses the problem of an alternative equilibrium analysis of unemployment theory. For instance, Milgate (1982, p. 8) states that "The constructive core of the <u>General Theory</u> is seen to consist in the provision of an argument that holds, unlike the orthodox marginalist position, that unemployment arises not through the operation of 'temporary phenomena', but rather that it is the outcome of the operation of more systematic and permanent forces at work in the system." Milgate and Garegnani have their own idea about the further question of

how we should pursue this argument. Unfortunately, in purging Keynes of his orthodox method, they seem to have lost sight of (indeed, reject), his views on interest and money (Milgate, 1982, pp. 102-124). It will be our purpose to follow out the first concern with an alternative framework for understanding unemployment equilibriums, but to do so by preserving the insight of Keynes' monetary views. This is the role the own-rates structure of chapter 17 can play in understanding the contemporary literature and the historical record.

We now have in full view the complex of issues which form the Gordian Knot that the "own-rates" theory will (hopefully) cut. From the strictly historical viewpoint, we have seen that much of the complexity and meaning of Keynes' interest rate theory was lost in the development of postwar monetary theory. In retrospect, Keynes' own post-General Theory defense of this part of his thought provided a foreshadowing of its eventual eclipse when his work was forced into the structure of a Walrasian general equilibrium setting (remember that Hicks was one of the authors he was complaining about in Keynes, 1937c). But the recognition of this loss by Keynes scholars starting (at least) with Leijonhufvud's famous reinterpretation has not brought these ideas back to the mainstream of monetary thought. Our investigation of the Keynes scholarship literature reveals two facets of this failure. First, the "Austrian-style" interpretation of Keynes by Leijonhufvud led inexorably to an abandonment of Keynes' own monetary theory of the rate of interest. Second, the one group of scholars who do explicitly recognize and cultivate Keynes' views on money, uncertainty and interest, the Post

Keynesians, at the same time abandon any type of equilibrium theory at all, as being inadequate to the task of capturing the manifold influences on the liquidity preferences of the public. This extreme stand has made for an uncomprehending reception of Post Keynesian ideas by an analytically-minded economics profession.

The knife that could cut such a knot must then be one that finely balances all of these influences. It must embody Keynes' own concerns that uncertainty and the special character of money are essential to understanding the interest rate. It must clearly provide an alternative to the formidable body of theory that ascribes to the interest rate the role of equating the supply and demand for loanable funds (i.e., saving and investment). And it must do all of this by appeal to a method that channels the complicated influences of social convention, investor psychology and history into the centering framework of an equilibrium analysis. It will be our purpose for the rest of this essay to show that the own-rates doctrine can simultaneously cut each of these strands in our very formidable knot.

IV. SRAFFA: OWN-RATES IN A NONMONETARY ECONOMY

". . . Mr. Piero Sraffa, from whom nothing is hid" (Keynes, 1972 (1933), p. 97).

The central piece of evidence in support of the theme that the "own-rates" theory of interest represents both a substantial critique of the Wicksellian "natural-rate" doctrine, and also the foundation of a new approach to the place of money in economic theory is an obscure twelve-page review of Hayek's Prices and Production by Piero Sraffa in the Economic Journal for 1932. The place of this review in our theme is but a part of its larger historical setting in the development of interwar business cycle theory in general, and the transition of ideas in Cambridge economics from the Treatise on Money to the General Theory in particular. Sraffa's review represents a confluence of both of these intellectual tides by bringing to bear the latest (as of 1932) thinking of the theoretical debates of the "Cambridge Circus" on the system of thought propounded by Keynes' greatest rival to preeminence in business cycle theory at the time, F. A. von Hayek. That Sraffa's role in this whole debate should be so obscure yet (as we shall see) so powerful is further testament to the mysteriousness of his legendary academic career as a maieutic gadfly. The fascinating question of the role Sraffa might have played in the transition in Keynes' thinking at this crucial period and his neglect as a member of the Circus is a topic too far afield from our central concern. Hopefully, the eventual publication of Sraffa's papers, now underway, will shed some light on this. Whatever these may reveal, the own-rates theory is sure to figure largely in Sraffa's theoretical legacy.

More important to the present stage of the argument is to briefly touch upon the bare outlines of Hayek's version of the Austrian branch of Wicksellian "natural-rate" doctrine that Sraffa's insightful criticisms stem from. Hayek's Prices and Production (1935)[4] consists of a series of four lectures he gave at the London School of Economics in 1930. They were intended to outline the "Austrian" approach to the explanation of business cycles associated with the theories of Wicksell (1935) and Mises (1953).[5] Hayek's approach was a blend of the Austrian capital theory of Bohm-Bawerk, the short run quantity theory effects of the early British monetary theorists and the relative-price and interest rate theory of Wicksell. From a general standpoint, Hayek proposed to integrate the theory of relative prices with that of monetary theory. To this end, he focused attention away from the average (price level) effects of "monetary" disturbances toward the relative price effects (1935, pp. 28, 29). He was adamant in his rejection of the whole idea of an aggregate price level, much less its role as the focus of monetary theory; i.e., as in the traditional quantity theory approach.

Correspondingly, his explanation of business cycles centered on the effect of monetary policy on relative prices of capital and consumption goods. Here is the centrality of the Wicksellian natural-rate doctrine to his argument. In the Wicksellian scheme, the natural rate was the rate which equalized "the supply and demand for real capital" and if the bank rate (or actual rate) differed from this, the result was a divergence of "voluntary" saving and investments. It was the interest rate as an intertemporal price ratio between current (consumption)

production and future (capitalistic) production that signaled the investors to over-invest and thus create "forced savings." Crises and cycles, then, resulted from the inevitable destruction of capital that followed such a misdirection of production by bank-influenced prices.

Where Hayek followed Wicksell was in his focus on the natural rate (he used the term "equilibrium rate") as the rate which would equalize voluntary saving and investment as they would occur in a "nonmonetary" barter economy. The effect of monetary influences, in this scheme, operating as an intermediary between these two voluntary intertemporal decisions, was based on the banking system's ability to cause the money rate to diverge from this natural rate and thus misdirect production towards a more capital intensive mix (in Austrian parlance a "lengthening of the average period of production").

Where Hayek differed with Wicksell was over the role which the "natural rate" could serve as a guide to policy in an expanding economy. As an early Cambridge student of these interest rate theories puts it, there were

> . . . four tests which Wicksell gave for his "natural" rate: viz., (i) stabilization of the general price-level; (ii) equalization of current savings and investments; (iii) the identity of the "natural" rate with the non-monetary barter rate; and (iv) its identity, again, with the prospective yield on future real capital (Adarkar, 1935, pp. 30-31).

Of these four results of an actual rate being set equal to the natural rate (for Hayek by a "neutral" money policy of controlling the money supply), Hayek found a contradiction between (i) and the rest. In other words, he argued that while setting the natural rate equal to the

money rate would give expression to the voluntary decisions captured in (ii) through (iv), it could not at the same time keep the price level constant in an expanding economy:

> Nevertheless, it is perfectly clear that, in order that the supply and demand for real capital should be equalized, the banks must not lend more or less than has been deposited with them as savings. . . . And this means naturally that . . . they must never allow the effective amount of money in circulation to change. At the same time, it is no less clear that, in order that the price level may remain unchanged, the amount of money in circulation must change as the volume of production increases or decreases. The banks could either keep the demand for real capital within the limits set by the supply of savings, or keep the price level steady; but they cannot perform both functions at once (Hayek, 1935, p. 27).

Thus, Hayek stakes out his question. Since he views the goal of a "neutral" monetary policy (i.e., one that allows for the full expression of voluntary decisions over saving and investment) as a proper one, since giving full vent to individual decisions requires setting the money-rate equal to the natural rate, and since this policy is incompatible with attention to the general price level; then Hayek believes we must abandon any such fuzzy aggregative concepts as the price level and focus our attention on the relative price effects of a monetary policy:

> But it seems obvious as soon as one once begins to think about it that almost any change in the amount of money, whether it does influence the price level or not, must always influence relative prices. And as there can be no doubt that it is relative prices which determine the amount and direction of production, almost any change in the amount of money must necessarily also influence production (Hayek, 1935, p. 28).

In accordance with this emphasis on relative prices, Hayek saw his role as extending the Wicksellian framework to explain how cycles were the result of these relative price changes alone. Having set himself this goal, the rest of his book (and his other monetary work) was an

exercise of grinding out the extensive implications of any non-neutral policy on the multifarious interactions of an interrelated equilibrium system. Interestingly for us, he described this effort as a methodological shift of focus (away from the classical dichotomy) that was substantially similar in intent with Keynes' own concerns at the time:

> . . . I am of the opinion that in the near future, monetary theory will not only reject the explanation in terms of a direct relation between money and the price level, but will even throw overboard the concept of a general price level and substitute for it investigations into the causes of the changes of relative prices and their effects on production (Hayek, 1935, p. 29).

Keynes was calling at the same time for an integration of value theory with monetary theory. In a volume to which both Hayek and Keynes made contributions in this period, Keynes staked out his view of the future of business cycle theory in the form of what he described as a "monetary theory of production." For him, this meant a theory "in which money plays a role of its own and effects motives and decisions, and is in short one of the operative factors in the situation" (Keynes, 1933, p. 408).

But the emphasis on the integration of value theory and monetary theory is as far as the similarity between the research programs of the Austrian School and the Cambridge School of the 1930s can be pushed. From this common juncture, they diverge over the crucial questions of what distinguishes a monetary economy from a barter one, what is the fundamental nature of interest rates, and what is the peculiar influence of money. The Austrian approach, represented in this period by Hayek,

focused on abandoning "the superfluity of the concept of a general value of money" in order to concentrate on the intertemporal relationship of relative prices over time (Hayek, 1935, pp. 29-30). This is the theoretical counterpart to Hayek's insistent emphasis on the policy stance of a "neutral" money policy:

> The problem is never to explain any "general value" of money but only how and when money influences the relative values of goods and under what conditions it leaves these relative values undisturbed, or to use a happy phrase of Wicksell, when money remains neutral relatively to goods (p. 31).

The Cambridge approach, as we will try to show, is based on an attempt to trace out the monetary influences on the "real" factors of production through a conception of "the general value of money." In Keynes' work, the general value of money will become the "own-rate" on money and its peculiar effects on real production will both define his stance toward the monetary/barter distinction and towards the essential properties of money and interest. But in order to work out this integrated view of monetary and value theory, the Cambridge school had first to break with the Wicksellian conception of these matters. This is the theoretical juncture at which Sraffa's review of Hayek comes in. It breaks with the whole theoretical foundation on which Wicksell's "happy phrase" was built and looks forward to the "monetary theory of production" with which Keynes would replace Wicksell.

Sraffa's review begins with a general methodological criticism of Hayek's approach to monetary theory which clearly defines both his and Keynes' own approach. His criticism revolves around the conception, common to the whole Wicksellian tradition we have described, that in

order to investigate the properties of, and policies toward, monetary influences on production, one should begin with a hypothetical "barter" state and compare this ("natural," "real" etc.) system with an actual money system. As Sraffa indicates, the error of this method arises from the tendency to take the "real" barter state as the "natural" condition of equilibrium and to consider any deviation from this state as "distortions" caused by the "artificial" interference of the banks. In Hayek's case, this tendency shows up as a polemic for a predetermined policy conclusion:

> The starting-point and the object of Dr. Hayek's inquiry is what he calls "neutral money"; that is to say, a kind of money which leaves production and the relative prices of goods, including the rate of interest, "undisturbed," exactly as they would be if there were no money at all (Sraffa, 1932a, p. 42).

For Sraffa, Hayek falters from the start by making a state which is exactly as if "there were no money at all" the object of his inquiry into monetary theory and policy. His objections center on two heads. First, such a method could only serve as a policy inquiry into money if it took as its starting point various real monetary economies and then compared the results of disturbances to this system with the results in the nonmonetary economy. "This would bring out which are the essential characteristics common to every kind of money, as well as their differences, thus supplying the elements for the merits of alternative policies" (p. 43). Sraffa claims that Hayek ignores this point in his single-minded pursuit of "the wholly different problem of proving that only one particular banking policy (that which maintains constant under all circumstances the quantity of money multiplied by its velocity of

circulation) succeeds in giving full effect to the 'voluntary decisions of individuals'. . ." (p. 43).

But Sraffa has a further objection to the use of a barter state as the starting point of theoretical monetary analysis. He claims that by reducing a monetary system to a hypothetical barter state, the analysis ignores those very functions of money that define the problems of a monetary economy:

> The differences between a monetary and a non-monetary economy can only be found in those characteristics which are set forth at the beginning of every text-book on money. That is to say, that money is not only the medium of exchange, but also a store of value and the standard in terms of which debts, and all other legal obligations, habits, opinions, conventions, in short all kinds of relations between men, are more or less rigidly fixed (p. 43).

Since Hayek's starting point of a hypothetical "neutral" barter state considers only money which is "used purely and simply as a medium of exchange," his inquiry can easily ignore the "most obvious" effects that a monetary policy will have in a real money economy, where

> when the price of one or more commodities changes, these relations [described above] change in terms of such commodities; while if they had been fixed in commodities, in some specified way, they would have changed differently, or not at all (pp. 43-44).

Thus, by confining him to his hypothetical world, Hayek's method "amounts to assuming away the very object of the inquiry."

In essence, the problem is that while imagining a system in which there is no money deprives us of our practical foundation in knowledge of a money economy, the opposite method of defining a money economy which we understand and then removing various aspects of money to define its "moneyness" builds on the concrete, not the speculative. This attitude

is a common thread of the Marshallian tradition to which Keynes (and Sraffa?) and the later Cambridge school are heirs. It differs radically from the Austrian subjectivist lineage of Hayek. It was not in Marshall's nature to make heroic leaps from hypothetical "barter economies" to modern monetary policy conclusions.[6] But at least for the Bohm-Bawerkian line of Austrians, the starting point of economic theory was an intricately worked out system of subjective states of mind. In this sense, Keynes' whole revolution consisted of a more thorough-going Marshallianism than Marshall himself, taking as his starting point the given state of unemployment and trying to theoretically explain it. Even today, the converse case of taking the state of full employment as the starting point of analysis and looking for deviations from this benchmark to explain unemployment remains the standard procedure among both Austrian and neoclassical economists. What Sraffa was pointing out in 1932 was that the burden of efficacy for such a position must rest beyond observation, which the Cantabridgians could always fall back on.

Given this methodological critique at the beginning of Sraffa's review, the rest of it can usefully be viewed as an application of what we have described as the "Cambridge" method to the same problems Hayek is interested in investigating. Two of these, in particular, will form the basis for Sraffa's idea of own rates of interest. They are the concept of intertemporal transactions in the hypothetical moneyless barter state, and the notion of a "natural-rate" of interest that equilibrates the flow of savings supplied by the public with the demand for investment funds by the entrepreneural sector. Explicitly at issue here is the whole

Wicksellian scheme of monetary analysis of which Hayek was a devoted follower and also the issues that will later appear in Keynes' General Theory as the "essential properties" of interest and money. The common thread in both treatments is that "taste for the concrete" as the starting place of analysis that is the hallmark of both Sraffa's and Keynes' approach, here embodied in the idea of own-rates of interest.

> Dr. Hayek's theory of the relation of money to the rate of interest is mainly given by way of criticism and development of the theory of Wicksell. He states his own position as far as it agrees with Wicksell's as follows: --"In a money economy, the actual or money rate of interest may differ from the equilibrium or natural rate, because the demand for and the supply of capital do not meet in their natural form but in the form of money, the quantity of which available for capital purposes may be arbitrarily changed by the banks" (Sraffa, 1932a, p. 49).

For Sraffa, the conception of the relation between the theory of money and the theory of prices (or conversely, the conception of the difference between a barter and a monetary economy) implied by this Wicksellian theme represents the heart of the problem with the "natural-rate" doctrine:

> An essential confusion, which appears clearly from this statement, is the belief that the divergence of rates is a characteristic of a money economy: and the confusion is implied in the very terminology adopted, which identifies the "actual" with the "money" rate, and the "equilibrium" with the "natural" rate. If money did not exist, and loans were made in terms of all sorts of commodities, there might be at any one moment as many "natural" rates of interest as there are commodities, though they would not be "equilibrium" rates. The "arbitrary" action of the banks is by no means a necessary condition for the divergence; if loans were made in wheat and farmers (or for that matter the weather) "arbitrarily change" the quantity of wheat produced, the actual rate of interest on loans in terms of wheat would diverge from the rate on other commodities and there would be no single equilibrium rate (1932a, p. 49).

This is a complicated passage full of issues which can only be gleaned from the context of the rest of Sraffa's critique. First, it is important to note the attention to terminology because Sraffa's use of terms is a crucial aspect of his difference with Hayek. Recall that the Hayekian analysis revolves around the specification of a barter-like "neutral" monetary policy where neutrality is defined as a state which leaves "the relative prices of goods, including the rate of interest, 'undisturbed,' exactly as they would be if there were no money at all" (Sraffa, 1932a, p. 42). Further, recall that Hayek defined the rate of interest which would achieve this result as the natural or equilibrium rate. We will come back in Essay III to the very interesting question of whether or not Hayek and Sraffa have two different conceptions of "equilibrium" in mind here, but for the purposes of Sraffa's critique the term "equilibrium" refers to the Marshallian notion of long-period equilibrium where prices equal costs of production and uniform rates of return are realized on investments in all industries.

Utilizing this long-period "equilibrium" conception and taking at face value Hayek's argument that the "natural-rate" is indeed the rate that would obtain in a barter-like state, Sraffa asks a very obvious question: what would loans and interest rates look like if money did not exist, and how would they be different from rates of interest in a money economy? His answer: if we were really in a barter state, the only meaning that loans, savings, or investment could have would be defined in physical terms, money being nonexistent by definition. Thus, "natural" rates of interest would be rates defined in "real" or physical terms.

The ratio between the amount of a physical commodity today to the amount it trades for at some future date would be the physical analog to rates of interest. This is the germ of the conception of own-rates of interest. As to how such barter-rates differ from interest rates on money, Sraffa only hints that the essential difference is not captured by the Wicksellian schema. It will be left to Keynes in 1936 to fully draw out the implications of this idea for a monetary economy . Sraffa's role is mainly critical. But in these critical "years of high theory" at Cambridge, it was "escaping the old ideas" that formed the major hurdle.

Both Sraffa's question and his answer are ingenious examples of the subtle intellect of the man whom Keynes referred to as "Piero, from whom nothing is hid" (Keynes, 1972 (1933), p. 97). It seems to have been Sraffa's role to ask fundamental questions about matters that other theorists simply took for granted (what is the meaning of a supply curve?, what does "quantity of capital" mean?, etc.) and to get simply devastating answers ("we don't really know, but let's not admit it"). This case is no exception, for it is fair to say that the full import of Sraffa's question about the Wicksellian natural rate, though hardly recognized then or now, pulls the moorings from beneath the one central point around which a generation of business-cycle theorists, each hotly contesting the explanation to this central enigma of the time, all seemed to agree upon. Leijonhufvud, in an essay that attempts to revive this very Wicksellian theme, identifies almost everyone with this central idea:

In the generation following Wicksell, we find suites on his basic

> theme composed by the Swedish, by the Austrian, and by the Cambridge schools. Before the General Theory, it was the dominant theme in monetary and business cycle theory as an imposing parade of names will testify: Cassel, Lindahl, Ohlin, and Myrdal; Mises and Hayek; Hawtrey, Robertson, and Keynes were among those who put the theme squarely in the center of major works of theirs. After the General Theory, however, the theme is no longer prominent. It was abandoned by monetary economists and left to antiquinarians. So, what happened?

According to our view, Sraffa and Keynes are "what happened." Having asked his simple and seemingly innocent question of the Wicksellian scheme, Sraffa, with his characteristic "taste for the concrete," brings us down to cases. He claims that his barter rates are not as hard to imagine as we might think. "In order to realize this, we need not stretch our imagination and think of an organized loan market amongst savages bartering deer for beavers. Loans are currently made in the present world in terms of every commodity for which there is a forward market." It is at this point that Sraffa provides a practical illustration of the concept of the "natural" or "commodity"-rate which measures the intertemporal equivalents, in physical terms, between spot and future market transactions.

> When a cotton spinner borrows a sum of money for three months and uses the proceeds to purchase spot, a quantity of raw cotton which he simultaneously sells three months forward, he is actually "borrowing cotton" for that period. The rate of interest which he pays, per hundred bales of cotton, is the number of bales that can be purchased with the following sum of money: the interest on the money required to buy spot 100 bales, plus the excess (or minus the deficiency) of the spot over the forward price of the 100 bales (p. 50).

This simple idea is Sraffa's interpretation of barter rates of interest. They are simply interest rates on a loan defined in terms of intertemporal equivalents of the physical unit being loaned. This is

what Keynes means by "the marginal efficiencies of a good in terms of itself" (1937a) and "own-rates of interest" (1936 p. 223). It is Keynes' eventual use of the concept that is our main focus, but it will be instructive to follow Sraffa a bit further since his use of the concept is much more directly concerned with its implication for the Wicksellian natural-rate doctrine.

Armed with a concrete conception of what a rate of interest means in barter terms, Sraffa goes on to compare this conception with the Wicksellian scheme.[7] In such a world, equilibrium (in Sraffa's sense) means that "the spot and forward price coincide, for cotton as for any other commodity; and all the 'natural' or commodity rates are equal to one another, and to the money rate." But, if supply and demand get out of long-period equilibrium for any reason, the spot and forward prices diverge, and the "natural" rate of interest on that commodity diverges from the "natural" rates on other commodities. In other words, "equilibrium" rates are not "natural" if by equilibrium we mean prices equal cost of production and by "natural" we mean barter-like (money-less) intertemporal loans. Thus, Hayek's (and Wicksell's) attempt to equate the equilibrium rate with the putative natural rate represents an "essential confusion." For, as Sraffa comments in the exchange which followed the review, Hayek's policy prescription of equating the money rate with the natural rate can have no meaning in Hayek's own preferred situation of an expanding economy: "The only meaning (if it be a meaning) I can attach to this is that his maxim of policy now requires that the money rate should be equal to all these divergent natural rates"

(Sraffa, 1932b, p. 251).

In a sense, Sraffa has here caught Hayek in a trap of his own making and no amount of squirming can release him, short of abandoning some part of his Wicksellian scheme. Recall that Hayek explicitly wanted to address himself to an investigation of the effect of monetary influences on relative prices. He also claimed to have rejected any reliance on "vague" concepts of averages such as the price level. He was thus led to found his polemic for a neutral monetary policy on the cyclical influence of money on relative investment flows in an accumulating economy. His policy prescription was to eliminate these "forced savings" influences by setting the money rate of interest equal to the "natural" rate. Here, Sraffa has shown that if you really want to define natural rates in barter-like terms, that it is a necessary relative price effect that these rates will "naturally" diverge in an economy in which accumulation is going forward. The basis of Sraffa's argument is that any new accumulation is directed to different employments by divergencies of market prices from the "natural" price, a good Marshallian argument. "It will be noticed that, under free competition, this divergence of rates is as essential to the effecting of the transition [to a more capitalistic economy] as is the divergence of prices from costs of production; it is, in fact, another aspect of the same thing" (Sraffa, 1932a, p. 51).

Consequently, a theoretical investigation, within the framework of a natural rate that reflects a barter state of savings and investment, into the relative price effects of credit- (read bank-) induced forced savings must deal with the fact that "there may be as many 'natural' rates as

there are commodities" (Sraffa, 1932a, p. 50). Hayek must either give up a unique "natural" rate to use as a guide to policy, abandon the whole Wicksellian conception, or fall back on one of his hated aggregates to deliver himself from this quandry. Sraffa points out that within the Wicksellian framework, one way out would be to use as a policy guide ". . . a 'natural' rate of interest which, if adopted as a bank-rate, will stabilize a price-level (i.e., the price of a composite commodity): it is an average of the 'natural' rates of the commodities entering into the price-level, weighted in the same way as they are in the price-level itself" (p. 51). By this method, Wicksell could salvage his own system (although the "natural" rate would now be non-unique, changing with every change in the components of the price index) and could meet Hayek's criticism of the falling price-level effects. But Hayek himself cannot fall back on such a solution since he has already rejected the conception of an average price level as vague and useless.

It is at this point that we come full circle to the starting methodological position and we see that Sraffa's own view is that we should abandon the whole Wicksellian scheme. Having shown the only logically consistent meaning that can be attached to the theoretical and policy conclusions of the Wicksellian framework, Sraffa implies that the whole effort is a misguided one from the start.

> It appears, therefore, that these non-monetary economies [i.e., those defined by his composite "natural" rates] retain the essential feature of money, the singleness of the standard; and we are not much the wiser when we have been shown that a monetary policy is "neutral" in the sense of being equivalent to a non-monetary economy which differs from it almost only by name (Sraffa, 1932a, p. 51).

In other words, the distinction between monetary effects and real effects is not illuminated by the Wicksellian approach of defining the "natural" state of saving and investment in hypothetical "barter" terms, where barter means uninfluenced by the arbitrary actions of the banks. As Sraffa had commented at the start, the Wicksellians are not actually defining a barter state, so much as a hypothetical "real" system, where money can only serve as a medium of exchange ("being entirely unaware that it may be doubted whether under a system of barter the decisions of individuals would have their full effects. . . ."). Thus, by ignoring the more important distinctions between a monetary and nonmonetary economy, "those characteristics which are set forth at the beginning of every text-book on money," the Wicksellian approach provides a misguided framework for a monetary theory.

In effect, what Sraffa has done here is to repeat his standard procedure of accepting a theoretical framework and then pushing it to its furthest logical extremes to see what insights it might yield. In the case of the "natural rate" view of monetary theory, what he found was a gaping emptiness beneath a thin skin. There is no unique natural rate that will stabilize the price level and ensure against forced savings in an accumulating economy. The essential properties of interest and money are ignored when we use a state in which money serves none of its multifarious cultural, conventional roles as the starting point of monetary theory. And the central problems of savings, investment and business cycles are not approached by this artificial distinction since "with or without money, if investment and saving have not been planned to

match, an increase in saving must prove to a large extent 'abortive'" (Sraffa, 1932a, p. 52). This last sounds like the "Circus" speaking, in its echo of the General Theory. And it is to the General Theory that the constructive use of the idea of own-rates leads us.

As a bridge over the uncharted, and perhaps forever unknown, question of what personal role was played by Sraffa in the eventual form of Chapter 17 of the General Theory, we can utilize the thoughts of a Cambridge student of the early thirties, a disciple of Keynes, Adam Smith Prizeman and eventually Professor of Economics at the Benares Hindu University, Mr. Bhalechandra P. Adarkar. Adarkar's The Theory of Monetary Policy (1935) provides an interesting contemporary survey of the major monetary theories of the thirties and constructive work on the usefulness of this body of theory for practical monetary management.

Besides the fact that Adarkar provides a concise discussion of the complete range of interest rate theory at the time, he forms an important intellectual link in our study by virtue of his knowledge of Keynes' monetary theory in the transition period from the Treatise on Money to the General Theory. Keynes' influence is clearly stated in Adarkar's preface:

> It will not be difficult for the reader, however, to discern the intellectual genealogy of this effort and he will at once perceive how deeply indebted I am to J. M. Keynes, that leader of modern monetary thought, in much that I have to say in the following pages (p. viii).

From this, we can feel confident that Adarkar's view will reflect the Cambridge school's view on monetary matters. For our purposes, two chapters are especially interesting: Chapter seven on "Mr. Sraffa's

Commodity Rate," and chapter eleven titled "Is Barter Theory Relevant?"

In the latter of these two, Adarkar sets out to investigate "the question as to what extent the theory of a non-monetary economy is likely to be useful in the understanding of monetary phenomena." He notes that the concept has been much used by modern writers on monetary problems, but that "the attitude of most writers in this matter has been altogether dubious, if not misleading." "Cassell, Wicksell and Hayek" are explicitly mentioned as cases where "hypothetical considerations of a non-monetary character have been introduced in monetary theory in connection with our problem [the idea of a natural rate]." His approach to the question is to consider a number of different conceptions of barter since these "hypotheses. . . are not in pari materia but relate to distinct concepts of the non-monetary economy" (1935, p. 86). Adarkar distinguishes between two conceptions of a barter economy. The first is a Robinson Crusoe economy used by Bohan-Bawerk and Rosher to illustrate capital accumulation. This economy is not really barter so much as exchangeless, and here the acts of investment and saving actually occur in physical terms. More interesting for our purposes is what Adarkar calls "an advanced social economy, in which there is exchange but no medium of exchange, in which goods are exchanged against goods" (p. 87).

Hayek's ideal barter state is identified with this money-less economy where "only those investments can be carried out which are justified by the available real savings. . .[which] avoids the disparity between savings and investments, resulting from our adherence to money" (p. 89). But the assumption that that such a moneyless state would be

more stable than a monetary system, Hayek's thesis, is questioned by Adarkar. He points out that "relative valuations of goods, services and other forms of wealth are liable to fluctuate therein as much as they do in a money economy." In fact, he thinks that such a nonmonetary economy would be less stable since all of the factors making for disruption, "viz., psychology, natural and physical phenomena, discoveries and so forth are present," but the monetary factor, "the only one that implies some sort of control," is "absent" (p. 88). As we will see, this closely reflects Keynes' own ideas about a barter state.

Due to this instability, and because focusing on barter phenomena encourages us "to forget that what we are primarily concerned with is money itself and its mysterious interactions on the processes of production, distribution and consumption," Adarkar finds such nonmonetary systems of little use in either monetary theory or policy. Interestingly for our thesis, he connects this rejection with an abandonment of the whole natural-rate doctrine utilizing Sraffa's review of Hayek. He says that even if we accept the automaticity of savings and investment in the barter state, "it does not help us to ascertain the ideal rate of interest that should be adopted under the money system." Relying on Sraffa's example, he points out that even in the barter state, the natural rate is not unique but varies with the number of commodities considered. "Moreover, even if we succeed in constructing such an average 'barter' rate, we have no reason to suppose that that rate, because it secured the savings-investment equalisation, under barter, would do so here also; . . . It is for this reason, among others,

that we have to reject the very ingenious concept of the 'natural rate'. . . ." (p. 90). Is barter theory relevant? To a Keynes' disciple of 1935:

> There is no need, however, to suppose that a return to barter would mean the elimination of all the economic problems arising from changes in relative valuations. True, money sometimes distorts the vision and puts false appearances on the realities of economic life and thus necessitates our probing deeper, viewing kaleidoscopically what is happening in the realm of realities. But this is not the same thing as to visualize the modern money economy as a mere disfigured replica of its cruder ancestor (Adarkar, 1935, p. 91).

V. KEYNES: OWN-RATES IN A MONETARY ECONOMY

In chapter 17 of The General Theory, Keynes lifts Sraffa's barter rates (redubbed by Keynes as own-rates) out of that "disfigured replica of its crude ancestor" and sets them down in a modern money economy. So far, we have established the corrosive effect that Sraffa's commodity-rates approach represents to the whole Wicksellian natural-rate, loanable funds framework. We need now to pick up the development of its more constructive uses in illustrating Keynes' liquidity preference theory. Once again, Adarkar proves a useful bridge in that in the same book in which he disparaged the usefulness of the natural-rate doctrine, he also reviewed "Mr. Sraffa's Commodity Rate" (1935, pp. 41-44). Working without the benefit of the General Theory and missing entirely the ironic critique implied by Sraffa's use of own-rates, Adarkar finds little use for the concept in his own concern with monetary policy. He seems to have thought that Sraffa was in fact proposing that his average commodity rates be adopted as a policy guide. Nevertheless, the very naivete of his criticisms, coming as they do from a student of the Keynes of the Treatise, provides a useful transition to Keynes' own use of the concept.

Adarkar's chapter is mainly focused on the possible use of Sraffa's average commodity rate as a standard by which the banking authorities might set the money rate. To this task, he finds it inadequate due to its instability. What is particularly interesting, though, is that Adarkar critiques the bare concept of the commodity rates from the standpoint of the rich institutional detail about monetary markets of

Keynes Treatise on Money. Again, he foreshadows, I think unintentionally, many of the issues that will appear in Keynes' own use of this concept.

Adarkar points out that Sraffa's commodity rates bear a strong resemblance to Fisher's attempt to reckon "real" rates of interest[8] "for each separate commodity by correcting the money rate for a change in its new spot price" (Adarkar, 1935, p. 42). But the difference between them, he notes, is that Fisher wanted to consider the difference between two spot prices separated in time while Sraffa's rate is completely calculated on current market evaluations as reflected in simultaneously existing spot and future prices. Thus, Fisher's real rate "is a de facto affair, on which monetary policy could only hold a post mortem"; but Sraffa's rate "is a living fact on which we could rely for active guidance in that if disequilibrium arises, the forward prices indicate it" (p. 42).

It is this forward-looking aspect of Sraffa's commodity rates that peculiarly adapts them to Keynes' preferred mode of monetary theory: uncertainty about the future. And, of course, what Sraffa implied in his review was that precisely those functions of money which relate to intertemporal situations were the ones Hayek and the natural rate theorists were ignoring at the peril of relevance to a "real monetary economy." Adarkar, although still confused about Sraffa's use of the concept, details the implications such psychological aspects of money hold for commodity rates.

First, he mentions that in a risky market the equilibrium position of spot and future prices will not exactly coincide due to the "cost of hedging." "As Mr. Keynes has shown (1930, vol. 2, p. 143), in equilibrium the spot price exceeds the forward price, the 'backwardization' amounting to as much as 10 per cent in the case of seasonal crops" (p. 43). For Keynes, this normal backwardization is subsumed under one element of his conception of own-rates, the liquidity premium. Adarkar further notes that a true reckoning of such rates must take into account "costs of warehousing, insurance [and] deterioration" of the stocks held over; "the speculative element" in spot and forward dealings; and the "current ideas and expectations of business men as to the probable course of future production" (p. 44). Each of these influences, we will see, also has a counterpart in the own-rates framework. While for Adarkar these influences made it "questionable whether we could depend upon such data to discover the norms and equilibria of industry," in Keynes' hands these concerns will constitute a "monetary theory of production" based on both real and monetary influences. It may be that an implication of this theory is that there are no "norms and equilibria" by which we can regulate the economy through monetary policy, as was the goal of the natural rate theorists. But in terms of explicating Keynes' central concern with interest and money in defining unemployment equilibriums theoretically, Adarkar foreshadows Keynes' own argument, to which we now turn.

Chapter 17 comes 222 pages into a complicated theoretical attempt to define unemployment equilibria as the normal case of a capitalist

economy. As many of his pre-publication correspondents and post-publication critics have commented, its discussion runs on a more general plane of reasoning from the more closely argued model that precedes it.[9] Yet, it obviously must be understood as a part of that greater work. For this reason, it is prudent to preface our detailed investigation of chapter 17 with a brief look at Keynes' argument up to that point.

Keynes' theory of the equilibrium positions of the economic system was "general" in its insistence on the possibility of a range of outputs and employments being consistent with the normal functioning of the system. Keynes felt that his essential conclusion was that a less-than-full employment equilibrium was the normal case within this possible range. In arriving at this conclusion, he felt that the fundamental analytical breakthrough in his own thinking had been the realization of "the psychological law that when income increases, the gap between income and consumption will increase" (1973b, p. 85). It was from this simple idea that he derived his fundamental building blocks of the multiplier and the theory of effective demand. Keynes thought that the neglect of aggregate effective demand, or "demand for output as a whole," had made the classical theory irrelevant except in the special case of full employment. His analysis of their argument in chapter 2 attributes this neglect to a reliance on the second classical postulate and Say's Law. For Keynes, the way to exhibit such a range of equilibriums was to throw out the second postulate and to supply the missing equation for effective demand.

Because that part of income that is not spent is not necessarily directed to productive use, the level of aggregate demand fluctuates. The result is uncertainty over future levels of activity. To Keynes, this meant his aggregate demand theory would have to deal with saving and investment activity in an uncertain environment. Hence, the fascinating discussions of expectations for which The General Theory should be famous.

Keynes situated the main effect of this uncertainty in the investment activity of business. Having cut the strict productivity moorings from beneath the classical theory of interest, it was necessary for Keynes to provide an alternative formulation. Thus, the last element in his system was the liquidity preference theory of interest. This filled the gap in a manner consistent with his analysis of the psychological uncertainty of investment behavior.

In brief, given short-period conditions, output and employment depend upon aggregate effective demand, which is wagged up and down by fluctuating investment behavior (the consumption function being stable). A complex of productivity, monetary and expectational conditions, all packed into the portmanteaux of the liquidity preference function and the marginal-efficiency-of-capital schedules, determines investment behavior and thus output and employment. According to Keynes, when it is realized that output and employment are not givens, but are uniquely correlated with the level of effective demand, the practically obvious existence of involuntary unemployment can be theoretically explained. It is only in the context of this argument that his definitions of involuntary

unemployment and full employment can be understood.

Keynes left it to chapter 17 to fully draw out his theory of interest and money for reasons clearly set out in the first paragraph of that chapter:

> It seems, then, that the rate of interest on money plays a peculiar part in setting a limit to the level of employment, since it sets a standard to which the marginal efficiency of a capital-asset must attain if it is to be newly produced. That this should be so, is, at first sight, most perplexing. It is natural to enquire wherein the peculiarity of money lies as distinct from other assets, whether it is only money which has a rate of interest, and what would happen in a non-monetary economy. Until we have answered these questions, the full significance of our theory will not be clear (Keynes, 1936, p. 202; my italics).

Here, we find all of the questions which were focused on in the Hayek-Sraffa debate: the money-barter distinction, the general property of interest and the peculiar property of money interest. But now these questions will not be addressed within the Wicksellian framework that Sraffa found so unsatisfactory, but in Keynes' world. Accordingly, we can look to this analysis for a synthesis of Keynes' concern to deal with money in an uncertain environment (as detailed in part III) with the definition of unemployment equilibria already set out in the General Theory.

In synthesizing a general theory of interest and money that is compatible with his general theory of employment, Keynes draws on Sraffa's commodity rates as an exploratory tool, now following out the implications of this notion for what Sraffa termed "real monetary economies." Methodologically, this involves Sraffa's subtle admonition to Hayek about starting from concrete situations as well as Adarkar's

less subtle references to the failing of commodity rates to capture the full institutional detail of a monetary economy. Though we have gone far afield from Keynes, coming back to roost in chapter 17 will provide (at least) one way to understand Harrod's comments about Keynes' central concern with a "wrong rate of interest" that is "not itself a rigidity or inflexibility," but is "natural, durable, and in a certain sense, in the free system inevitable" (1947, p. 70). To use Keynes' own words, "Until we have answered these questions the full significance of our theory will not be clear."

Keynes (1936) immediately gets down to concrete cases, introducing commodity-rates as a natural definition of interest on both money and other types of assets:

> The money rate of interest--we may remind the reader--is nothing more than the percentage excess of a sum of money contracted for forward delivery, e.g. a year hence, over what we may call the "spot" or cash price of the sum thus contracted for forward delivery. It would seem, therefore, that for every kind of capital-asset there must be an analogue of the rate of interest on money. For there is a definite quantity of (e.g.) wheat to be delivered a year hence which has the same exchange value today as 100 quarters of wheat for "spot" delivery. If the former quantity is 105 quarters, we may say that the wheat-rate of interest is 5 per cent. per annum; and if it is 95 quarters, that it is <u>minus</u> 5 per cent. per annum. Thus for every durable commodity we have a rate of interest in terms of itself. . . ." (p. 222).

Footnoting Sraffa, Keynes works through an example, similar to Sraffa's cotton spinner, of the wheat-rate of interest. He defines such commodity rates as "own-rates" and notes that "there is no reason why their rates of interest should be the same for different commodities," since the relations of spot and future prices for different commodities are "notoriously different" (p. 223). Thus, in the most general context

of the own-rates conception, "the money rate of interest has no uniqueness compared with other rates of interest, but is on precisely the same footing" (p. 225).

Yet, although as an intertemporal market equivalent there is no difference between a money transaction and a wheat transaction, Keynes implies that the money rate is unique for two interrelated reasons. The money rate is the standard in which all other future values are contracted for and estimated, and there are peculiar reasons why the money rate may be less flexible downward than other rates. If we can show why these two properties adhere to money rather than other assets, we will have some justification for using money as the standard in which to measure the marginal efficiency of capital, and for using the money rate as the marginal efficiency which "rules the roost" in the sense of providing a rate which other assets must attain to be newly produced. In other words, it is the fact that money is just another asset and that it is a peculiar asset that warrants attention on the money rate as the regulator of investment.

In order to get to the rest of the differences and similarities between the range of observed assets and own-rates of interest, Keynes introduces a scheme of attributes which defines the relative desirability of different assets, essentially a demand equation for assets. This scheme addresses Adarkar's comments (based on the Treatise, remember) on the specific failings of the commodity-rates in a monetary economy as well as illustrating Keynes' liquidity preference theory. It is through this abstraction that Keynes relocates the own-rates from the Wicksellian

world to his own.

Taking "various commodity-rates of interest over a period of (say) a year" and measuring each rate "in terms of itself" as the standard of intertemporal value, Keynes finds "three attributes which different types of assets possess in different degrees." These are:

> (i) Some assets produce a yield or output q, measured in terms of themselves, by assisting some process of production or supplying services to a consumer.
>
> (ii) Most assets, except money, suffer some wastage or involve some cost through the mere passage of time (apart from any change in their relative value), irrespective of their being used to produce a yield; i.e., they involve a carrying cost c measured in terms of themselves. . . .
>
> (iii) Finally, the power of disposal over an asset during a period may offer a potential convenience or security, which is not equal for assets of different kinds, though the assets themselves are of equal initial value. There is, so to speak, nothing to show for this at the end of the period in the shape of output; yet it is something for which people are ready to pay something. The amount (measured in terms of itself) which they are willing to pay for the potential convenience or security given by this power of disposal (exclusive of the yield or carrying cost attaching to the asset), we shall call it liquidity--premium l (pp. 225-226).

In Keynes' scheme, an asset's own-rate of interest will be defined by "its yield minus its carrying cost plus its liquidity premium." Recalling Adarkar's specific complaints about commodity rates, we can now see that in Keynes' definition, we have so far taken account of "costs of the stocks held over" with c, "current ideas and expectations of business men as to the probable course of future production" with q (and to some extent l), and "the speculative element" with l. But Adarkar's further concern with the relation of pure commodity rates to the Fisher effect is also addressed by Keynes. It is worthwhile to look at this relationship

a bit more closely since it sheds light on the important and interesting question of the relation of interest rates and inflation in the own-rates framework.

Keynes claims that so far as the relationship between spot and future prices on different commodities reveals a multitude of own-rates, that any of these commodities which are held for investment purposes could conceivably be used as the standard in which to measure the marginal efficiency of capital assets (recalling that his concern is with explaining investment demand):

> For we can take any commodity we choose, e.g. wheat; calculate the wheat-value of the prospective yields of any capital asset; and the rate of discount which makes the present value of this series of wheat annuities equal to the present supply price of the asset in terms of wheat gives us the marginal efficiency of the asset in terms of wheat (p. 224).

According to Keynes, the choice of standard is arbitrary so long as "no change is expected in the relative value of two alternative standards." We are very close here to the Fisherian doctrine of the effect of expected inflation on current interest rates. The relative value of two alternative standards is just Fisher's "appreciation of money" where we are, as Keynes says (p. 227), "taking money (which need only be a money of account for this purpose, and we could equally well take wheat) as our standard of measurement. . . ." In this situation, it is possible to correct for differences in both different standards over time or in relative changes with respect to one particular standard. In the case of choosing a standard, the whole structure of "own-rates" moves up and down "when one of the alternative standards is expected to change

in value in terms of the other." The influence of Sraffa is echoed in this context by the concern over variant and invariant standards of value. Basically, there are two problems involved. First, there is the question of which good to use as the standard in which to measure all relative own rates (including the all important marginal efficiency of capital) today. As Keynes said above, this choice is arbitrary in the sense that we can value any expected stream and current price in terms of any standard we choose, e.g., wheat. "If no change is expected in the relative value of two alternative standards, then the marginal efficiency of a capital-asset will be the same in whichever of the two standards it is measured, since the numerator and denominator of the fraction which leads up to the marginal efficiency will be changed in the same proportion" (Keynes, 1936, p. 224).

But if one of the standards is expected to change in value (appreciate), "the marginal efficiencies of capital-assets will be changed by the same percentage, according to which standard they are measured in." Keynes illustrates his conception of the effect such an appreciating standard will have on the structure of own-rates by a simple example where "wheat, one of the alternative standards, is expected to appreciate at a steady rate of _a_ per cent. per annum in terms of money." According to Keynes, in this simple case _a_ will provide an additive adjustment factor to the marginal efficiency of an asset to distinguish rates determined in one standard or another with the ranking of asset values remaining unaffected:

> The marginal efficiency of an asset, which is x per cent. in terms of money, will then be x-a per cent. in terms of wheat. Since the marginal efficiencies of all capital-assets will be altered by the same amount, it follows that their order of magnitude will be the same irrespective of the standard which is selected.

Keynes' simple example of a standard appreciating at a steady rate skirts a great complication here, in that the real problem will arise when different assets appreciate at different rates in terms of different standards. In other words, relative rates of appreciation are not all to be expected to be the same in terms of any particular standard. Fisher was well aware of this problem when he noted: "There are, therefore, theoretically just as many rates of interest expressed in terms of goods as there are kinds of goods diverging from one another in value" (1930, p. 42). His solution--not surprisingly, coming from the master of the theory of index numbers--was to express his "real rate" of interest by adjusting the money rate with ex post changes in a cost of living index.

Keynes himself was no fool when it came to index number problems (see Treatise, book II), and his collaboration with Sraffa along with the interminable difficulties of capital theory controversies in his day, seem to have made him justifiably wary of aggregate indices of all kinds. This wariness (as well as Sraffa's influence?) shows up in the present context in his comment on defining a standard:

> If there were some composite commodity which could be regarded strictly speaking as representative, we could regard the rate of interest and the marginal efficiency of capital in terms of this commodity as being, in a sense, uniquely the rate of interest and the marginal efficiency of capital. But there are, of course, the same obstacles in the way of this as there are to setting up a unique standard of value.

In accordance with this rejection of a composite standard, Keynes includes an appreciation factor "a" in his equations for own-rates which take into account "what the changes in relative values during the year are expected to be" in order to "determine the expected returns on different types of assets which are consistent with equilibrium" (p. 227). By this method, Keynes avoids the problems of a "unique standard of value," while retaining price expectations in his demand for asset scheme. The "a" terms also serve the function of converting the diverse commodity own-rates into a single (but not unique) standard of value. Thus, where

$$q_i - c_i + \ell_i$$

defines the commodity own-rate in real terms for commodity i, the same rate measured in money terms will yield the "money rate" of commodity i,

$$q_i - c_i + \ell_i + a_i \quad .$$

A question immediately arises which can only be dimly seen at this stage. What has become of the Fisher effect here? Essentially, it has been dethroned from Adarkar's strictly algebraic post mortem of interest rates that have already happened and replaced with a living, current expectation. In the process, Keynes has also abandoned any attempt to theoretically define an index measure of the different expected appreciation rates of different assets by different people. There will, nevertheless, be a market effect of these expectations in terms of different own-rates being brought into equality in money terms by the additive "a" terms. But, as we will see, since Keynes' views on financial markets place great weight on the diversity of opinions about

the future course of asset values, this effect is not well-captured by an index number approach. What will be important will be the "shifting" of these market valuations which will reflect a change at the margin of opinion (i.e., some of Keynes' famous "bears joining the bull brigade").

From the standpoint of the Fisher effect, the main element of this expected appreciation will reflect uncertainty about the future ability of the monetary standard to fulfill its role as the most liquid asset. As we will see when we get to Keynes' views on the essential properties of money, this will involve Fisher's interest in the stability of prices in money terms to some extent. But the effect of instability will not be a strictly additive factor in valuing interest, but a complicated matter of social convention. By this view, Keynes' ultimate Fisher effect is more drastic than Fisher's own. For Keynes, a lack of confidence in the socially conventional standard is more qualitative than quantitative. If instability detracts from money's role as the liquidity standard, some substitute for liquidity will be set up by the organic methods of market evolution. But a full understanding of these issues requires more detail of the own-rates framework.

Returning to the development of the own-rates framework of asset market equilibrium, it is useful to digress a bit and consolidate the argument so far. What we have seen is that the bare concept of a commodity rate introduced by Sraffa implies that, from the most general standpoint, a money rate of interest is not unique. Just like a money transaction, any intertemporal market trade implies an own-rate of exchange between spot and future quantities. To be absolutely clear

about this deceptively simple idea, it is convenient to resort to some algebra. In this context, we will start from (and then elaborate on) the useful algebraic formulation of own-rates of interest by Joseph Conard (1959, pp. 119-154). Although Conard is led astray in his discussion by failing to account for Keynes' views on uncertainty, his formal discussion of the concepts of commodity rates and money rates is the clearest exposition in the literature.

Conard begins his investigation by defining own-rates as a case of the Fisherian "rate of return over cost" (p. 120):

$$\frac{\text{Expected future value - present value}}{\text{present value}}$$

In this framework, "real" or "commodity" own-rates (which we will designate as r_c) are defined in quantities (Q) of the physical commodity

$$r_c = \frac{Q_2 - Q_1}{Q_1} .$$

Denominated in a standard of value, the same rate will become a money-denominated own-rate in value terms (which we will define $r_{c,m}$):

$$r_c = \frac{P_2Q_2 - P_1Q_1}{P_1Q_1} .$$

By this method, Conard shows that Keynes' "a" terms, the difference between real own-rates and money own-rates, are inherently complex

$$a = r_{c,m} - r_c$$

$$= \frac{P_2Q_2 - P_1Q_1}{P_1Q_1} - \frac{Q_2 - Q_1}{Q_1}$$

$$= \left(\frac{P_2 - P_1}{P_1}\right)\left(\frac{Q_2}{Q_1}\right)$$

So the "a" that transforms a real own-rate into a money own-rate is the expected proportionate price change times the intertemporal quantity equivalents.

We can use this formulation to clarify a number of issues. First, recall that Sraffa (1932a, p. 50) had defined the commodity rate as being equal to the money rate "plus the excess (or minus the deficiency) of the spot over the forward price." This is the definition referenced to Sraffa by Keynes that he used in working out his wheat example. It provides a clear link between the abstract concept and market phenomena. Using Conard's framework, we can specify this link more clearly. The relationship postulated by Sraffa is

$$r_c = r_m - \left(\frac{P_2 - P_1}{P_1}\right)$$

where: r_c = the commodity own-rate

r_m = the money own-rate

P_2 = the future price of the commodity

P_1 = the spot price of the commodity

It is noteworthy in this context that Keynes defines equilibrium in the asset market as the situation where all own-rates, defined in a common standard, are equal. "Thus in equilibrium the demand-prices of houses and wheat in terms of money will be such that there is nothing to choose in the way of advantage between the alternatives" (p. 227). Using money as the standard, this means that all money own-rates and the money-

rate itself will be equal in equilibrium; or, for all commodities i,

$$r_c^i + a^i = r_m .$$

In terms of the above definitions,

$$r_m = \frac{M_2 - M_1}{M_1}$$

$$r_c^i = \frac{Q_2^i - Q_1^i}{Q_2^i}$$

$$a^i = \frac{P_2^i - P_1^i}{P_1^i} \left(\frac{Q_2^i}{Q_1^i}\right) ,$$

by which we get a more precise version of the Sraffa formula:

$$r_c^i = r_m - \left(\frac{P_2^i - P_1^i}{P_1^i}\right)\left(\frac{Q_2^i}{Q_1^i}\right)$$

This not only confirms the Sraffian lineage of Keynes' concept, but it also makes clear that Keynes' definition of equilibrium in this context is strictly a current, short-period affair. To see this, recall that Sraffa, using the long-period as his definition, defined "'equilibrium' rates as the situation where spot and forward prices coincide . . . and all the 'natural' or commodity rates are equal to one another, and to the money rate" (Sraffa, 1932a, p. 50). But here, Keynes has defined his "equilibrium" as an equality between all own-rates measured in money terms which, as the above formula makes clear, could easily be a disequilibrium in the long period sense. This may be one way of understanding Keynes' insistence that the difference between a monetary and a real economics entails a specification of the "line of division between the theory of stationary equilibrium and the theory of

shifting equilibrium--meaning by the latter the theory of a system in which changing views about the future are capable of influencing the present situation. For the importance of money essentially flows from its being a link between the present and future" (p. 293).

In this view, Keynes' "shifting equilibrium" is defined by a monetary equilibrium on the asset market where all own-rates consistently measured are equal, but spot and forward prices still diverge according to current expectations of productivity, carrying costs and liquidity. Alternatively, Sraffa's equilibrium commodity rates are part of the theory of "stationary equilibrium." In a formal sense, this is captured by Sraffa's view that equilibrium requires all own-rates measured in quantity terms to be equal, and all spot and future prices to coincide. This is why the details of Keynes' "a" terms are so important. It is the movements of the prices on spot and future markets that guarantee his equilibrium position will exhibit a market configuration of equal expected money-denominated own-rates for every asset. But to see why Keynes' equilibrium shifts with "changing views about the future," we need to investigate Keynes' second question of Chapter 17. What is it about money as an asset that makes it unique?

In terms of his schema of attributes of assets, Keynes distinguishes money by its high liquidity premium and low carrying cost:

> . . . it is an essential difference between money and all (or most) other assets that in the case of money its liquidity-premium much exceeds its carrying cost, whereas in the case of other assets their carrying cost much exceeds their liquidity premium (p. 227).

The full implications of the special character of money, though, only

become apparent in terms of Keynes' argument about the relationship between his shifting equilibrium on the asset market and the level of employment. His argument at its most general level follows the discussion of the post-General Theory writings reviewed earlier by focusing on the level of interest rates as the determinant of new investment spending. In the own-rates context, investment is disaggregated into individual capital-assets which are both newly produced and traded on second-hand markets. At any given time, the outstanding stocks of both physical and pecuniary assets will be valued by the market in accordance with the demand for each asset's individual attributes of productivity, carrying cost and liquidity. Given that the stocks are slowly adjusted, the price established on the second-hand market will determine, when compared with the "normal supply price," in what directions and amounts investment flows proceed.

The definitions of equilibrium and the movements implied between equilibrium positions are very poorly specified by Keynes. At some junctures, his argument involves defining instantaneous stock equilibriums and at others discussing flows of production of investment goods. In fact, to make sense of his argument, it is necessary to bring in a number of elements that define a very complicated picture only hastily sketched by Keynes. An analytical interpretation of this framework is possible if we center our attention on Keynes' equilibrium own-rates as a market phenomenon around which his complicated story operates. By this interpretation, a stock equilibrium configuration of asset returns can serve as a focus of both liquidity preference theory

and the discussion of money.

To begin, we have Keynes' assertion that equilibrium in the asset market will be characterized by a state where all own-rates, measured in a single standard, will equal each other (1936, pp. 227-228). It is in this context that he defines the "a" terms for each asset as the expected rate of appreciation of that asset in terms of the standard. Lerner (1952, pp. 173-179) and Conard (1959, pp. 120-134) show that when reduced to any common standard, the rate of interest on all assets is necessarily equal since any deviations (abstracting from risk and term differences) would provide arbitrage opportunities that would drive the prices of the assets into such an equilibrium. Thus, in the context of asset market equilibrium, the "a" terms can be seen as the necessary positions of supply and demand equilibrium in spot and forward markets that ensure that all assets yield an equal return when consistently measured. This is clearly shown in the form given to the a's above where they are defined by the difference between spot and future prices:

$$a^i = \frac{P_2^i - P_1^i}{P_1^i} \left(\frac{Q_2^i}{Q_1^i}\right)$$

It is Keynes' scheme of motives for holding different assets that provides the underlying economic forces which drive the demands for various commodities. Productivity, costs and liquidity considerations shift these demands between the various stocks of assets; but, on the market, the price configuration given by the relationships of the money rate and the spot and forward money prices is driven toward an equilibrium defined by the a's. Conard (1959) provides clear examples of

this in the form of a variety of assets, each of which has different commodity own-rates, but all of which yield an equal rate when measured consistently in any of the standards. Abba Lerner (1952) provides a similar analysis in his insightful interpretation of the own-rates theory and concludes:

> The wheat rate of interest and the money rate of interest are not automatically equal by definition: they are only brought into approximate equality in equilibrium by arbitrage in perfectly competitive markets.

To use Keynes' example where there are 3 assets--money, houses and wheat--we get 3 individual money-rates of own-interest all measured in money as the standard. The wheat rate is due to its predominant physical characteristic of high carrying cost and is primarily held for an expected rise in its price:

$$r_{w,m} = a_1 - c_1 = \frac{Q_2^w - Q_1^w}{Q_1^w} + \frac{P_2^w - P_1^w}{P_1^w}\left(\frac{Q_2^w}{Q_1^w}\right)$$

The house rate is due primarily to its productive capacity to generate services, q:

$$r_{h,m} = a_2 + q_2 = \frac{Q_2^h - Q_1^h}{Q_1^h} + \frac{P_2^h - P_1^h}{P_1^h}\left(\frac{Q_2^h}{Q_1^h}\right)$$

The money rate is defined by the unique character of money that "its yield is nil, and its carrying cost negligible, but its liquidity premium substantial" (p. 226).

$$r_{m,m} = \ell_3 = \frac{M_2 - M_1}{M_1}$$

Using this framework, the asset market equilibrium condition is defined

by

$$r_{w,m} = r_{h,m} = r_{m,m}$$

or,

$$a_1 - c_1 = a_2 + q_2 = \ell_3$$

or,

$$\frac{Q_2^w - Q_1^w}{Q_1^w} + \frac{P_2^w - P_1^w}{P_1^w}\left(\frac{Q_2^w}{Q_1^w}\right) = \frac{Q_2^h - Q_1^h}{Q_1^h} + \frac{P_2^h - P_1^h}{P_1^h}\left(\frac{Q_2^h}{Q_1^h}\right) = \frac{M_2 - M_1}{M_1}$$

Many writers have commented on the stock equilibrium quality of Keynes' analysis, by which we mean that equilibrium is defined for a market evaluation of an existing quantity of capital, money bonds etc. Kenneth Boulding was so taken with this aspect of Keynes' approach that he proposed A Reconstruction of Economics (1950) based on the sole use of stock rather than flow equilibrium theory. G. L. S. Shackle (1967, p. 145) has commented that this use of stock analysis is particularly evident in Keynes' interest rate theory:

> One more of the great changes in outlook of economic theoreticians stands largely to Keynes' credit, and again it is largely a case where an idea or practice of Marshall's was radically deepened and enlarged. Marshall had compared the existing with the desired total stock of money, and proposed to regard the latter as proportional to national income. This was perhaps the first turning of the tide against the neoclassical emphasis on flows in contrast with stocks. Keynes' theory of the interest-rate fused method and meaning inseparably in a purely "stocks" analysis. It is the essence of the liquidity-preference theory that stocks and not flow are in command, and in stating this theory Keynes showed a "stocks" analysis at work.

It is interesting that Shackle mentions Marshall in this context since, as we have seen above, the very definition of "equilibrium" own-rates by Sraffa and Keynes, respectively, hinges on the choice of

defining them in the Marshallian long period (for Sraffa) or short period (for Keynes). Consequently, Sraffa looked to flows of resources between industries to eventually equalize all own-rates and the money rate by equalizing the spot and future prices of every commodity in a long period equilibrium. But Keynes, more alive to the financial realities of a complex money economy, relies on the arbitraging of wealth owners and speculators to drive the relation of spot and future prices of the outstanding stocks of assets into a configuration today that reflects current expectations about the desirability of each in the never realized future. As we will see, the importance of money in this scheme will revolve around its use as the link between these current expectations and the level of own-rates on the market today. It will be through the own-rate on money defined by its liquidity premium (along with the expected return on capital) that all of Keynes' fascinating discussion of long-term expectations and non-Benthamite opinions about the future that we discussed in part three will enter into the framework of the asset-holding equilibrium of chapter 17. Two more elements of the framework of analysis must first be dealt with.

Although Keynes defines asset equilibrium on the market by price movements on spot and forward markets, this is in a sense only the observable surface phenomenon around which the really interesting aspects of his story are centered. In fact, the major virtue of the own-rates analysis of Keynes' interest rate theory may be that it does provide such a manageable focusing device for the complex considerations he wanted to discuss in his interest rate theory, the same complexities that we have

argued are ignored in the "Keynesian" vision. It is not idle to speak of the market equilibrium rates as a "centering" device in this context since the complete story involves movements that occur both beneath this market equilibrium, in the form of individual decisions about expected asset values, and above this equilibrium in the form of flows of newly produced assets that respond to the market-determined prices. Before continuing, it is necessary to briefly specify each.

Keynes' analysis of the interest rate in chapters 13 and 15 of the General Theory is addressed to the determination of the rate of interest on money in modern financial conditions. At the root of the argument is the question of how an individual with a given level of savings out of income (the determination of which he had already distinguished from financial markets per se, by his aggregate demand analysis) will be held. In the simplest terms, Keynes formulates the decision as one between holding savings in the form of "immediate, liquid command" over goods and services versus being "prepared to part with immediate command for a specified or indefinite period, leaving it to future market conditions to determine on what terms he can, if necessary, convert deferred commands over specific goods into immediate command over goods in general" (p. 166). The extent to which wealth holders prefer one type of asset over another is the definition of their current state of liquidity preference. Defining this preference as a demand for money (in excess of that required for active circulation)[10] and the rate of interest on money as the price of parting with this liquidity, money interest becomes the "'price' which equilibrates the desire to hold wealth in the form of cash

with the available quantity of cash."

But a further question remains. Why is it that anyone would want to hold wealth in a form that yields a rate of return less than other financial instruments? Why does such a thing as liquidity preference exist? Keynes argues that the fundamental condition giving rise to a liquidity preference is "the existence of uncertainty as to the future of the rate of interest." This, of course, is the source of the famous "bootstrap" critique of Keynes whereby it is uncertainty over the future rate that determines the current rate of interest (Robertson, 1940; Hicks, 1946, pp. 163-164). From the standpoint of the complexity of the own-rates structure, this is shown to be a specious argument in that all of the multifarious elements of productivity, costs and time preference that the bootstrap critique considers left out of Keynes' argument are present here. Yet, in another sense, the "bootstrap" formulation goes to the heart of the contextual necessity for interest on money: uncertainty over future prices. But this should not be a criticism in the world Keynes was interested in describing. As Keynes repeatedly emphasizes, in his world actors are uncertain about the future, and the expectations that they hold about the future are just what the bootstrap critique implies: "hoist by their own petard."

What we are driving at is the central theme that Keynes emphasized as the distinguishing feature of his General Theory in the 1937 Q.J.E. article (Keynes, 1937c), namely the "conventionality" of expectations in an uncertain environment. As Keynes emphasized in that article, this conventional quality of expectations enters into the economic scheme with

particular force through asset valuations, both financial and capital. In terms of the own-rates structure, both types of expectations about profitability of investments and movements of financial prices will influence the equilibrium configuration through various q's, a's and ℓ. But expectations will be especially relevant to the discussion of the peculiarities of money since ". . . uncertainty as to the future course of the rate of interest is the sole intelligible explanation of the type of liquidity preference . . . which leads to the holding of cash" (p. 201).

Since our emphasis is on the use of the own-rates theory as a tool in understanding the liquidity preference theory, it would take us too far afield to discuss it in detail. But two elements that are laid out in chapters 12, 13 and 15 of the General Theory are worth emphasizing for the insight they lend to our understanding of chapter 17. Keynes emphasized in these discussions that the interest on money, arising as it does out of a desire for liquidity in the face of uncertain expectations about the future course of capital asset values, rests on a conventional judgment of what the future course of interest will be. For this reason, financial markets: (1) are "made" by the simultaneous existence of a variety of opinions; and (2) are subject to precipitous swings when the fabric of the conventional judgment is weakened. A few lengthy quotations from Keynes (1936) demonstrate the importance of these points:

> . . . the rate of interest and the price of bonds have to be fixed at the level at which the desire on the part of certain individuals to hold cash (because at that level they feel "bearish" of the future of bonds) is exactly equal to the amount of cash available for the speculative motive. Thus, each increase in the quantity of

> money must raise the price of bonds sufficiently to exceed the expectations of some "bull" and so influence him to sell his bonds for cash and join the "bear" brigade (p. 171).
>
> . . . It is interesting that the stability of the system and its sensitiveness to changes in the quantity of money should be so dependent on the existence of a variety of opinion about what is uncertain (p. 172).
>
> . . . Changes in the liquidity function itself, due to a change in the news which causes revision of expectations, will often be discontinuous, and will, therefore, give rise to a corresponding discontinuity of change in the rate of interest. Only, indeed, in so far as the change in the news is differently interpreted by different individuals or affects individual interests differently will there be room for any increased activity of dealing in the bond market (p. 198).

Now the tie-in between these two crucial aspects of liquidity preference theory--the necessity of a diversity of opinions and the liability of reevaluations in conventional judgments to effect sea changes in interest rates--and the own-rates theory has two consequences. First, the argument about the diversity of opinions provides the link between the equilibrium market structure of the own-rates when measured in a common standard with the scheme of individual judgments as to the expected productivity, liquidity and appreciation of various assets. In other words, in order to "make a market" for the variety of individual assets, it is necessary that individual estimates of the q's, a's, and * differ among individual investors. This is what we meant earlier by the individual decisions that go on beneath the structure of the equilibrium market configuration of asset prices.

Thus, by this argument the asset market equilibrium configuration implied by the own-rates structure is "built up" from a sophisticated microfoundation. The sophistication lies in the explicit recognition of

the social level influences on individual behavior, in the form of the market opportunities available to wealth holders and the social conventions underlying their future expectations. Further elaboration of this kind of (neglected) microfoundations would involve the role of different classes or "ideal types" of transactors on the markets, each with different goals and constraints. Two strong-type examples of this are evident in the Cambridge tradition of dividing bond holders up into two groups: widows and orphans and freewheeling speculators. R. F. Kahn (1954) makes much of this in his view of liquidity preference theory and notes that it links up the Keynes of the General Theory with the "two views" of the Treatise. More recently, work on the social foundations of the idea of rational expectations has come back to this point (Frydman, 1982; Frydman et al., 1982). Interestingly for us, the focus has been on the possible instability of a rational expectations equilibrium in its resemblance to the "Holmes-Moriarity problem," which is formally the same problem as Keynes' famous beauty contest (see O'Driscoll and Rizzo, 1985, pp. 84-85). In the own-rates equations for asset demand, this underlying conception of individual evaluations would actually imply a different equation for each different asset for each different agent. With n assets and m traders, we would get mn own-rate equations in the most general case. The market equilibrium rates would be equivalent to evaluations of the marginal traders only.

The second implication of these views of Keynes' is that this underlying variety of opinion, making up both sides of the market for the total stock of existing assets, can move rapidly between various

evaluations of the future (Keynes' "bearishness and bullishness" of the Treatise) because of alterations in the skein of conventional judgments upon which such evaluations exist. Keynes goes so far as to suggest that if all opinions about the future course of prices were unanimous and held with certainty, that a complete revaluation of assets could occur without any change in holdings whatsoever:

> If the change in the news affects the judgment and requirements of everyone in precisely the same way, the rate of interest (as indicated by the price of bonds and debts) will be adjusted forthwith to the new situation without any market transactions being necessary (p. 198).

The confluence of these two points is the determination of the shifts in Keynes' "theory of shifting equilibrium" to which we previously equated the "own-rates" theory. Assets are held in expectation of gain based on conventional judgments of the future by different individuals. This precarious equilibrium is liable at any time to "shift" when the foundation of current opinion about the future is disturbed. The magnitude of the shift will depend on the extent to which it is shared and the length of time it takes for a new convention to be established. Thus, our asset market "equilibrium" is seen to be simultaneously a fragile balancing of individual opinions, and one firmly based in the economic motives of personal gain (here, though, only personally defined) by investors and speculators seeking their own advantage.

The macroeconomic importance of this asset equilibrium, now shown to constitute a market element with an underlying microstructure, is the effect that the level of the shifting stock equilibrium has on "flows" of investment and hence employment. It is here that the last element of

Keynes' "vision" of chapter 17 comes in. These flows are the other side of the spectrum of which the own-rates form the center. For the result of the "highly conventional" phenomenon of asset market equilibrium has a very real effect in governing the desirability of investments in labor-employing projects. As Harrod's statement implied, it is for perfectly natural reasons that the rate of interest is "wrong." If in an uncertain world there is no reason to expect asset prices to reflect purely real employment opportunities, then the interest rate:

> . . . may fluctuate for decades about a level which is chronically too high for full employment: --particularly if it is the prevailing opinion that the level established by convention is thought to be rooted in objective grounds much stronger than convention, the failure of employment to attain an optimum level being in no way associated, in the minds either of the public or of authority, with the prevalence of an inappropriate range of rates of interest (p. 204).

This was the conclusion of Keynes' basic theoretical model of employment developed in the General Theory of which the liquidity preference function was an integral part. In chapter 17 with the use of the "own-rates" theory, Keynes shows that it may be that it is the very nature of money that causes this situation. In so doing, he addresses the further points of discussion in our Wicksellian theme: the nature of money, what a nonmonetary economy would look like, and whether there is such a thing as a "natural" rate of interest, which would avoid this problem altogether.

With our notion of asset market equilibrium in mind, we can follow Keynes into his discussion of the uniqueness of money in Sections II and III of chapter 17. It is interesting to note that he addresses the

question to a situation that was firmly established as the starting point of business cycle theory in the Wicksellian literature of his time. In particular, recall that Hayek (and Mises before him) wanted to discuss the natural rate in the context of an economy in which accumulation was going forward. Likewise, for Wicksell the goal was to try to use the marginalist method to explain this situation, using the tools of a long period value theory where factors all earned an equal rate of return. Keynes' innovation was to bring the financial side into this scheme and let his asset market equilibrium configuration determine equal "financial" rates of return at any given time. This is consistent with his severing of the savings-investment link that formed the basis of the Wicksellian story, where the interest rate equated real flows of savings with real flows of investment at the natural rate. Starting from this conception, the Wicksellian and loanable funds theorists would investigate the consequences of an upward shift in the investment schedule which set off new investment at the existing market rate of interest. In Wicksell, Mises and Hayek, the result of such a shift depended on whether the market rate was allowed to move to a new higher "natural rate" that would equilibrate saving and investment. According to this story, it was because the banking system could hold the rate beneath the natural rate, that we get "forced savings," an intertemporal misallocation of resources and an eventual crisis (see Leijonhufvud, 1981, pp. 151-160).

Keynes bases his analysis of the employment-generating effects of the own rates equilibrium on a similar situation. He asks what would be

the limiting factor that brings an increased production of new capital goods to a standstill? In his scheme, where the secondhand markets for goods continually revalue the whole stock of assets, the flow of new capital goods is determined by a comparison of the market-established rate of return on the existing stock with the expected marginal efficiency of new projects. In price terms, Keynes describes the comparison in terms of a "demand price" for capital goods which is fixed by discounting expected future streams of income from an investment back to the present using the market rate of interest (determined by the own-rates equilibrium). This demand-price is then compared to a supply-price which represents the marginal cost of producing that asset. If the demand-price exceeds the supply-price, new capital goods will be produced:

> Now those assets of which the normal supply-price is less than the demand-price will be newly produced; and these will be those assets of which the marginal efficiency would be greater (on the basis of their normal supply-price) than the rate of interest (both being measured in the same standard of value whatever it is).

Once in this Wicksellian situation of accumulation, where does the process stop? For Keynes, it stops when some asset's own-rate refuses to decline as accumulation goes forward and so holds up the market equilibrium rate of interest. The importance of declining own-rates is impressed upon Keynes by his capital theory. Basically, he assumed a declining marginal efficiency of capital assets as production of them expanded in the short run. The best discussion of this process is found in chapter 11 where the "Marginal Efficiency of Capital" is explicitly addressed:

> If there is an increased investment in any given type of capital during any period of time, the marginal efficiency of that type of capital will diminish as the investment in it is increased, partly because the prospective yield will fall as the supply of that type of capital is increased, and partly because, as a rule, pressure on the facilities for producing that type of capital will cause its supply price to increase (p. 136).

To link this disaggregated, expectations-based view of capital with the asset market that the own-rates represent, Keynes further emphasizes in chapter 16 that the failing of technical capital theory lies in ignoring the fact that a capital-asset is just another potential rate of return to investors in a modern economy. The classical argument that it is the physical productivity of capital that sets the pace of investment

> . . . overlooks the fact that there is always an alternative to the ownership of real capital-assets, namely the ownership of money and debts; so that the prospective yield with which the producers of new investment have to be content cannot fall below the standard set by the current rate of interest (pp. 212-213).

This reasoning is the basis of Keynes' contention that the source of the return on capital is not that it is productive, but that it is scarce:

> . . . the only reason why an asset offers a prospect of yielding during its life services having an aggregate value greater than its initial supply price is because it is <u>scarce</u>; and it is kept scarce because of the competition of the rate of interest on money (p. 213).

All of this attention to Keynes' theories of money interest and capital serves not only to link up the own-rates framework with the larger work of which it is a part, but also prepares our way toward understanding Keynes' attribution of uniqueness to money as an asset. In a way that Keynes does not explicitly point out (but that is implied by the grouping of chapters in book IV), his argument for the uniqueness of

money assets is an integral combination of his capital theory and liquidity preference theory. As we will see, the uniqueness of money is that it is only a very imperfect capital-asset; but that its peculiarities from a capital theory standpoint are just those qualities which make it desirable as a liquid asset. Perfectly in accordance with Keynes' views on money and capital, then, the own rates theory brings the analysis of financial and real assets under one framework.[11]

In the simplest terms, the marginal efficiency of each capital-asset, d, will be defined by that rate of interest which, when used to discount a future stream of expected returns from an investment, will just equal the current supply-price of that capital asset. If P^{s^i} is the present supply price of capital-asset i, then solving the following for d will yield the marginal efficiency of that capital asset.

$$P^{s^i} = \sum_{j=1}^{n} \frac{R_j^i}{(1+d)^j}$$

where: P^{s^i} = the present supply price of capital asset i;

R^i = the expected future stream of returns for each period j;

d = a rate of discount.

In terms of the own-rates, this d will equal the expected own-rate for capital asset i:

$$d = q_i + a_i$$

Then in equilibrium, those traders at the margin of preference between the different assets will determine an equilibrium value of r where for

all assets (i - 1, ..., n)

$$r = q_i - c_i + \ell_i + a_i$$

By Keynes' simplified example, we can reduce this to our 3 assets (wheat, houses and money) where the equilibrium interest rate (measured in money terms) becomes

$$r = a_1 - c_1 = q_2 + a_2 = \ell_3 .$$

In this context, the importance of the declining own-rates as accumulation proceeds revolves around which rate will hold up the decline of the others. Since they must "necessarily equal" on the market, the downwardly rigid rate will provide the level to which the others fall.

> As the stock of the assets, which begin by having a marginal efficiency at least equal to the rate of interest, is increased, their marginal efficiency (for reasons, sufficiently obvious, already given) tends to fall. Thus, a point will come at which it no longer pays to produce them, unless the rate of interest falls pari passu. When there is no asset of which the marginal efficiency reaches the rate of interest, the further production of capital assets will come to a standstill (p. 228).

The question is, which of the own-rates will be the stubborn one that holds up the decline? Keynes thought there were certain "peculiarities" of the money rate which made it the own-rate that is reluctant to fall as output increases. The uniqueness of money as an asset revolves around the employment-generating effects that we have ascribed to the own-rates market equilibrium, and which we have seen flow from Keynes' views on capital. It is because capital-assets can be produced that they ultimately fall in value (their own-rates decline) as

accumulation proceeds. The first unique characteristic of money is that it cannot be so readily produced in response to changes in its price:

> Thus, the characteristic that money cannot be readily produced by labour gives at once some prima facie presumption for the view that its own-rate of interest will be relatively reluctant to fall; whereas if money could be grown like a crop or manufactured like a motor car, depressions would be avoided or mitigated because, if the price of other assets was tending to fall in terms of money, more labour would be directed into the production of money (pp. 230-231).

But since this zero elasticity of production is also satisfied by any other pure rent factor fixed in supply, this cannot be the sole uniqueness of money. "The second differentia of money is that it has an elasticity of substitution equal, or nearly equal, to zero; which means that as the exchange value of money rises, there is no tendency to substitute some other factor for it." Keynes' argument here is that since the only reason money is held is for its liquidity value, that a change in the relative value of money will not have an adverse effect on its desirability as an asset. This is because the liquidity premium of a unit of money is only increased by an increase in its relative value (a decrease in prices). "This follows from the peculiarity of money that its utility is solely derived from its exchange value, so that the two rise and fall pari passu, with the result that as the exchange value of money rises, there is no motive or tendency, as in the case of rent-factors, to substitute some other factor for it." If, like other rent factors, the desirability of the money-asset fell as its price rose, then an increased demand for it would "slop over into a demand for other things." Then the demand for money as an asset would at least indirectly reach a point of calling forth new employment. Since this is not the

case, there is the possibility that money could become "a bottomless sink for purchasing power" (p. 231).

From the standpoint of capital theory, then, the two peculiar qualities of money are that demand for it cannot call forth new production directly as in the case of capital goods proper, or even indirectly through substitution of other factors as in the case of pure rental items. For these two reasons, the avenues by which movement in capital-asset own rates is accomplished, by new production, are closed off to the money asset.

But what of other liquid goods (e.g., wheat) of which it may not be possible to immediately increase the supply in response to an increased demand? Why couldn't the wheat rate of interest hold up all the other rates? This is where Keynes' second attribute of assets to hold, carrying-costs, comes in. Recall that Keynes had earlier defined money by its quality of possessing the highest excess of liquidity-premium over carrying-cost. The importance of the low carrying-cost of money is that it sharply distinguishes money from all other potentially liquid assets (as opposed now to productive ones). Here, the distinction rests not on the supply side but on the demand side. In the case of all other liquid assets, the advantages offered through holding increased stocks of them are sharply limited by the cost of holding them for any appreciable time. Thus, "although a larger stock might have some attractions as representing a store of wealth of stable value, this would be offset by its carrying-costs in the shape of storage, wastage etc." But this is not so in the case of money where "the readiness of the public to

increase their stock of money in response to a comparatively small stimulus is due to the advantages of liquidity (real or supposed) having no offset to contend with in the shape of carrying-costs mounting steeply with the lapse of time" (p. 233).

Keynes uses this argument to explain why he thinks the effect of falling prices on the "effective supply" of money will not offset the position of money as a bottomless sink of purchasing power. In essence, he is anticipating what would become the "real balances" effect made so much of by Patinkin. He asks if it might not be the case that the stagnating influence of the high money-rate would be offset by an "effective" increase in the supply of cash? The increased cash that resulted from falling prices (a reduction in the wage-unit) would operate via two avenues:

> a reduction in the wage unit will release cash from its other uses for the satisfaction of the liquidity motive; whilst, in addition to this, as money-values fall, the stock of money will bear a higher proportion to the total wealth of the community (p. 232).

Keynes disputes the argument that these effects would satisfy the increased demand for liquidity and thus negate the dominant position of the money rate in setting the pace of investment. First, he claims that the important reaction to a fall in the wage unit concerns "the <u>difference</u> between these [capital assets' own-rates] and the money rate of interest." It could be that the decline in wages would be even worse in creating an expectation of further declines and thus decreasing the marginal efficiency of capital. Secondly, he cites his frequent point that "the fact that wages tend to be sticky in terms of money, the money-

wage being more stable than the real wage, tends to limit the readiness of the wage unit to fall in terms of money." And in fact, due to the major place of wages in the expectations of future demand that hold up the marginal efficiency of capital, this stickiness is beneficial on the whole. Thirdly, "the most fundamental consideration in this context" is the characteristic of money's high liquidity-premium over carrying-cost which makes it possible to absorb extra quantities of money without facing extra costs. As we will see, these qualities of wage stickiness and low carrying cost are Keynes' explanation for money's liquidity. Thus, both the capital theory aspects and the liquidity characteristics of money combine to give the money rate its "sting":

> The significance of the money-rate of interest arises, therefore, out of the combination of the characteristics that, through the workings of the liquidity-motive, this rate of interest may be somewhat unresponsive to a change in the proportion which the quantity of money bears to other forms of wealth measured in money, and that money has (or may have) zero (or negligible) elasticities both of production and of substitution (p. 234).

The effect on employment, then, operates through the level of equilibrium set by the necessary equality of the money-rate and all other own-rates, and the pace of investment demand this rate calls forth. When the money-rate is reluctant to fall, all other own-rates fall to its level and no further. "The money-rate of interest, by setting the pace for all the other commodity-rates of interest, holds back investment in the production of these other commodities without being capable of stimulating investment in the production of money, which by hypothesis cannot be produced." From the asset market view, the very existence of the social convention of money becomes Keynes' culprit for unemployment:

"Thus in the absence of money . . . the rates of interest would only reach equilibrium when there is full employment" (p. 235).

This is the point at which the capital theory argument for the importance of money as the asset which holds up the own-rates of all other assets joins the liquidity preference discussion of the essential properties of money. Ultimately, Keynes' attribution of importance to the money-rate rests on the qualities of money as an asset "which constitutes money as being in the estimation of the public, par excellence 'liquid'." In section IV, Keynes brings the argument full circle by considering "how far those characteristics of money as we know it, which make the money rate of interest the significant rate, are bound up with money being the standard in which debts and wages are usually fixed" (p. 236).

His consideration proceeds in two steps. "In the first place, the fact that contracts are fixed and wages are usually somewhat stable in terms of money unquestionably plays a large part in attracting to money so high a liquidity premium." It is because future debts and costs will be payable in money that money can perform its liquidity function, by definition. If the future standard of payments was not expected to be stable, then money would not be liquid.[12] But this very stability is dependent upon the low elasticity of production of the money-asset which caused the trouble with its rate of interest. It is also true, Keynes claims, that the low carrying-cost of money is important to its role as the standard of deferred payments. "For what matters is the difference between the liquidity-premium and the carrying costs." Even if the

public attached as high a liquidity premium to wheat by fixing contracts, in wheat terms the carrying-costs would nevertheless be so high that "the wheat rate of interest would still be unlikely to rise above zero" (p. 237).

The importance of the low carrying cost on money is the link between this consideration of money as the medium of exchange and a standard of deferred payments and its function as a store of value. Consequently, Keynes secondly considers the "subtle" fact that

> The normal expectation that the value of output will be more stable in terms of money than in terms of any other commodity, depends of course, not on wages being arranged in terms of money, but on wages being relatively sticky in terms of money (p. 237).

If this were not the case, and wages were "expected to be more sticky in terms of some one or other commodities other than money," then two requirements would have to be met by those commodities if they were to take our money's place as the dominant own-rate. First, they would have to have a constant cost relative to real-wages for any scale of output. Secondly, they would have to have a sufficiently low carrying-cost to allow any "surplus over the current demand at cost-price. . . [to be] taken into stock without cost." The first requirement guarantees that the good's relative value would remain stable over the short and the long run as production ebbs and flows. The second requirement ensures that any old stock of the commodity would not affect its value. If such a commodity could be found, it "might be set up as a rival to money" in its role as the most stable of all stores of value (p. 238).

Keynes did not think it was "probable that any such commodity exists," but from our theoretical vantage point it is interesting to note that these very requirements are strictly met by a fiat standard which Keynes always had in mind when he spoke of money. In that case, the cost of production is fixed (at nearly zero) and invariant to scale, and the costs of holding are as close to zero as possible (in its own terms). Keynes saw a duality of meaning between the predominating fact that that money is the standard of payment and its peculiar qualities as an asset:

> I conclude, therefore, that the commodity, in terms of which wages are expected to be most sticky, cannot be one whose elasticity of production is not least, and for which the excess of carrying-costs over liquidity-premium is not least. In other words, the expectation of a relative stickiness of wages in terms of money is a corollary of the excess of liquidity-premium over carrying-costs being greater for money than for any other asset (p. 238).

As Keynes says, "Thus we see that the various characteristics, which combine to make the money-rate of interest significant, interact with one another in a cumulative fashion." All of the qualities of money can now be seen as dependent on each other. Moreover, the centrality of the money-rate, resting as it does on the very properties which make money liquid, illustrates the essential unity of Keynes' monetary, capital and investment theory. Even further, the fact that the liquidity function of money ensures that wages and payments will be most stable in money terms provides a theoretical justification to the traditional "Keynesian" concern with the fixed money wage case. In recent years, this assumption has become the very symbol of ad-hoc theorizing with which economics should have no truck. While, in fact, Keynes' complete theory of unemployment does not rest on rigid money-wages,[13] his discussion of

money shows why he thought it was such an important case to treat.

The quality of sticky money wages is an assumption much closer to the reality of a money economy than the opposite assumption of Pigou-effects and real-balance-effects where money prices freely adjust to keep relative prices the same. Keynes emphasized this point in chapter 17 with reference specifically to Pigou and his "presumption in favour of real wages being more stable than money-wages." Keynes points out that with changes in employment (scale of output) and the high carrying-cost of wage goods, the stickiness of real wages would

> . . . cause a violent oscillation of money prices. For every small fluctuation in the propensity to consume and the inducement to invest would cause money-prices to rush violently between zero and infinity. That money-wages should be more stable than real wages is a condition of the system possessing inherent stability (p. 239).

In terms of our set-up of the own-rates market equilibrium, Keynes is emphasizing that the "a" terms would have to fluctuate wildly if prices were assumed to be the sole adjustment factor that equated own-rates to a fixed liquidity premium.[14] In other words Pigou's mistake ("in fact experience. . . and logic") is in assuming the operation of a "real balance" effect that would automatically readjust the nominal stock of money to provide the desired liquidity without affecting interest-rates. Such a rapid and complete adjustment to changing conditions of money demand would imply much less price stability than we in fact observe or than is compatible with the stability of the system. This argument clarifies Keynes' frequent assertion that even if wages and prices were perfectly flexible in a depression, that the effect on expectations of such instability (operating through expected q's of

capital assets in our own-rates) would make matters even worse.

But if real relative values are not the source of stability to the system, we are led to ask what does the amount of observed stability (is there any?) in the interest rate depend upon? This question is not taken up by Keynes, but its fascination led one of Keynes' students, Hugh Townshend,[15] to call for an amendment to the theory of value based on Keynes' analysis of a money economy. Townshend thought that chapter 17 represented "the most general theory" of Keynes' book (1973b, p. 258). "Thus, it would seem that Mr. Keynes' doctrine of liquidity-preference really involves a generalization of the classical (marginal) theory of value" (1937, p. 160). Specifically, the generalization that Townshend envisioned depended on extending value theory into the determination of "money prices" in an economy where goods are not just produced for immediate consumption but are also held for future security. In this context, the structure of relative prices will not be strictly determined "at the margin of production" (as in the classical theory) but will depend to some extent (depending on the degree of "moneyness" or liquidity of a good) on the psychologically determined liquidity premiums that attach to monetary assets. The psychological impacts of these liquidity premiums are explicitly referred to by Townshend as involving the distinction between the "exchange of existing assets (at the margin of exchange) and the production of new assets (at the margin of production)" (p. 160).

Emphasizing the role of expectations in this generalized theory of value, Townshend sees the stability of the system of money prices so

determined to depend on the existence of a stable convention. Here is the answer we are seeking to the necessary practical role of sticky money prices in lending stability to the system. Townshend shows us the extent to which Keynes followed out that subtle method of basing even his most highly abstract conclusions on an observed reality of the economy rather than a purely hypothetical system:

> Since in fact money-values do not fluctuate wildly in the short period (save in abnormal conditions with which we are not here concerned), they must be kept reasonably stable by some characteristic of our real world of which a realistic theory of prices must take account. It would seem that this characteristic must be either a conventionality of outlook causing stability of expectations as to the money-prices of durable assets of certain kinds, or else conventional maintenance of some degree of stability of the money-price of the only other exchangeable value, viz. labour--that is to say a conventionally stable wage-unit (pp. 161-162).

Townshend dismisses the contention that the quantity of money along with its velocity of circulation can stabilize prices since to a greater or lesser extent any money stock can support any price level. This is because there need not be much actual exchange to revalue the stock of existing assets if the opinions about their future value are unanimous enough. But since such upheavals of prices do not continuously occur (except in the case of pure asset markets), the stability must depend on a convention:

> . . . since the quantity of money does not determine "the"--or, rather, any--price level, no prices would be determinate at all, unless at least one money-value--the price of something--were determined by habit or convention.

The implications for economic theory are that no prices are strictly determinate since nothing is "absolutely determined by convention"

(except perhaps in a command economy) and that economic theory can only provide "approximately true" propositions about relative prices "which is the best we can hope for in an undetermined and shifting price-world" (p. 162). For Townshend, this casts a pall over any attempts at "dynamic theorising." If we can only base our value theory on the shifting sands of a liquidity convention, then our theory can only be specified for the duration of each individual convention and no longer.[16] This notion provides some rationale for Keynes' simplest model where money-wages are fixed and only the shortest short-period equilibrium is investigated.

Townshend's fascinating writings extend Keynes' views on interest and money in novel directions. But for our purposes, they also bring us back to the extent to which the own-rates theory represents a challenge to the Wicksellian framework for monetary theory. First, the emphasis on the conventional basis of interest and prices serves to illustrate that Sraffa's injunction on the use of barter theory as the starting point of monetary economics was much more than just polemical tactics. As Townshend makes clear, the very notion of a structure of prices in a world where expectations of the future influence actions today has to start from some basis in the facts of the situation:

> All exchange values are relative (ratios). If all possible sets of values in a community are to be comparable numerically, there must be a money of account--a common denominator to which the ratios are reducible. In a capitalist community--that is to say, one in which some people employ hired labor for future profit--people will also hold durable assets for future security. Even if there is no legal tender money, assets so held--whether goods or paper claims--will possess liquidity-premiums; and some claims and/or other assets will come to oust other assets . . . for the purpose of liquid holdings. We then have, in all essentials for the purpose of a theory of value, a monetary economy. . . . The generally accepted claims or

> goods will modify the values which they are used to measure and are already real money for the purposes of the theory of value. Thus the text-book conception of a barter as non-monetary economy has no place in a discussion of value. The theory of value in a capitalist economy is the theory of money-prices (Townshend, 1937, pp. 166-167, italics added).

In other words, if the essence of money involves its role as a link between the present and the future, a link that grows out of its acceptance as the social numeraire, then money can only fulfill its role if it is assumed that money will be worth something at that future date. Keynes has shown us that this sort of expectation, which the money rate of interest is based upon, ultimately rests on an agreed upon convention of the stability of money prices. In the Wicksellian framework, where the operative forces of the case are looked for in a strictly "real" side of the economy, all of this is ignored. For Wicksell and Hayek, the barter-like "natural" rate of interest is the starting point of investigation. But if Keynes is right about money, there is nothing at all natural about such a barter rate in a money economy.

This friction between Keynes' theory and the whole natural rate, loanable funds framework is finally drawn out in the last two sections of chapter 17. In these, he explicitly addresses himself to the question of what a nonmonetary economy would resemble and what meaning can be given to the idea of a natural rate of interest.

Recall that Sraffa had shown, and Adarkar had recognized, that the nonmonetary state in which the "natural" rate was supposed to rule could not really be conceived of in barter terms. As Sraffa commented in his review of Hayek, "It may be doubted whether under a system of barter the

decisions of individuals would have their full effects" (1932a, p. 43). What the Wicksellians really wanted to define was a hypothetical state in which the social contrivance of a medium of exchange existed, but where none of the accompanying intertemporal allocation problems that follow from the use of money in an uncertain environment encumber the decisions of individuals. To the natural rate theorists, this could be accomplished by simply controlling the money supply.

Starting from a much richer conception of the social functions of money, Keynes conceived of such a "so-called 'non-monetary' economy" in a much different way. The only meaning he could give to the idea was a situation in which no asset possessed that fundamental quality of possessing a liquidity-premium in excess of its carrying cost.

> There exists nothing, that is to say, but particular consumables and particular capital equipments more or less differentiated according to the character of the consumable which they can yield up, or assist to yield up, over a greater or shorter period of time: all of which, unlike cash, deteriorate or involve expense, if they are kept in stock, to a value in excess of any liquidity premium which may attach to them (p. 239).

Even in this case, Keynes' liquidity motive would enter in the relative evaluation of assets by wealth holders. Here, the liquidity would depend on the variety, stability and marketability of the goods which each asset is capable of assisting in the production of.[17] The rate of interest, then, would still be dependent upon the liquidity preferences of the public, illustrating the fact that money is a purely social-specific device.

> There is, clearly, no absolute standard of "liquidity" but merely a scale of liquidity--a varying premium of which account has to be taken. . . . The conception of what contributes to "liquidity" is a

> partly vague one, changing from time to time and depending on social practices and institutions (p. 240).

But if no exclusively liquid good exists, would interest rates be low enough to ensure full employment growth and accumulation? Keynes does not specifically say, but implies that if such a money-to-hold did not officially exist, that one would have to be invented! It is as if liquidity preference is a human desire so strong that it creates its own object. As an example, Keynes mentions "that in certain historic environments the possession of land has been characterised by a high liquidity-premium in the minds of owners of wealth." In the absence of a good money, land would make a suitable liquidity standard due to its low elasticities of production and substitution and due to the fact that its output is at least as stable and marketable as any other. Keynes speculates that this might account for the unusually high mortgage rates, in excess of the net productivity of yields, found in many agricultural economies.

If it were the case that some readily marketable good is socially "set up" as the liquidity standard in a nonmonetary economy, this answers the question of the efficiency of such a natural state. For land in these situations can be every bit as inhibiting of production and accumulation as money is today:

> That the world after several millenia of steady individual saving, is so poor as it is in accumulated capital assets, is to be explained, in my opinion, neither by the improvident propensities of mankind, nor even by the destruction of war, but by the high liquidity premium formerly attaching to the ownership of land and now attaching to money (p. 242).

It was this very conception of a liquidity premium being a necessary social convention where wealth holding is a private matter, that Joan Robinson focused her attention on when she came to discuss "Own Rates of Interest" (1961). Already in her later classical political economy stage, this former Keynes student conceived of the problem of liquidity-premiums keeping up the rate of interest as a class-distribution issue. Defending Keynes against Kaldor's (1960) argument that land could not serve such a purpose because "the rise in the purchase price of land can lower its yield to any extent," and thus diminish its attractiveness, Robinson makes the important point that this ignores the fact that the liquidity premium is altogether different from a mere explicit return. As Keynes emphasizes, liquidity premiums are of the nature of his long-term expectations in the sense that they reflect uncertainty, not risk.[18] This is why he defines them as a "potential convenience or security. . ." for which there is ". . . nothing to show . . . at the end of the period in the shape of output; yet it is something for which people are ready to pay something" (1936, p. 226). Robinson takes up the idea of non-pecuniary yields from land as the liquid asset and fashions it into an historical explanation of the transition from a feudal state, represented by landed wealth, to a capitalist one, represented by capital wealth. Her argument adds "the pleasure of gentlemanlikeness derived from owning land" to the argument, but essentially applies Keynes' idea that some asset will always be set up as the liquidity standard by social convention. In an interesting twist on Keynes' main premise that the interest rate on money holds up the pace of investment, Robinson

speculates that, in the transition to capitalism, the traditional attribution of liquidity (and social distinction) to land holding may serve to hold up (primitive?) accumulation.

What she has in mind is an "'historic environment' (such as, indeed, exists in many countries today) when the capitalist wealth-owners exist side by side with gentlemen, whom extravagance and misfortune from time to time forces to pledge their ancestral estates." In this example, which corresponds closely to Keynes' hypothetical nonmonetary economy, the dissavings of the gentlemen will provide the original finance for industrial investment:

> Now, so long as land is known to be safe and sound while all industrial investment has a high risk premium, and when, as Keynes assumed, the return to be expected in each round of I [investment] is less than the last, lending to a gentleman will be a formidable rival to financing industry (Robinson, 1961, p. 590).

Robinson (1961) notes that this situation will be even worse in a social environment in which capitalists derive a further non-pecuniary "pleasure of gentlemanlikeness. . . from owning land." Also echoing Keynes, she thinks the problem could be long lasting if "capitalist wealth is diverted to purchasing land at second hand which . . . [if land yielded no non-pecuniary returns] would be more readily available to find an outlet in financing new investment." Besides providing an interesting theoretical explanation for the classical political economists' marked antagonism to the profligate ways of the landed aristocracy, Robinson shows that Keynes' framework is malleable enough to fit many social and historic environments. She even speculates that the role of take-over bids (in 1961 and now it would seem) provide a similar example of the

basic phenomenon of liquidity premiums holding up productive investment.

In order to bring our discussion back to our original starting point of the antagonism of Keynes' monetary views with the Wicksellian natural rate tradition, it is appropriate to point out that Keynes ends his own chapter on "The Essential Properties of Interest and Money" by explicitly dissenting from that view. Attributing the idea to Wicksell, Keynes notes that his own Treatise on Money used the idea of a natural rate "which preserved equality between the rate of savings. . . and the rate of investment." In doing so he had, "however, overlooked the fact that in any given society there is, on this definition, a different natural rate of interest for each hypothetical level of employment." In other words, savings always equals investment and the rate of interest, by determining the level of investment, just determines the level of employment for which the equality of saving and investment is defined.

> Thus it was a mistake to speak of the natural rate of interest or to suggest that the above definition would yield a unique value for the rate of interest irrespective of the level of employment. I had not then understood that, in certain conditions, the system could be in equilibrium with less than full employment (1936, pp. 242-243, italics added).

Keynes identifies the old Wicksellian concept as "merely the rate of interest which preserves the status quo," a rate which we really have no interest in defending. Declaring that it is not even a useful analytic category, he proposes to replace it with a neutral or optimum rate of interest. This more general concept would identify the rate of interest ". . . which is consistent with full employment, given the other parameters of the system" (p. 243).

With this concept in mind, we can see that Keynes' difference with the Wicksellian framework is not a mere choice over analytical frameworks (as Hicks (1937) would have it), but it is every bit as fundamental to his innovations on classical theory as the theory of aggregate demand. Just as in that case, the ultimate significance of Keynes' interest rate theory is its allowance for equilibriums consistent with less than full employment. Rejecting the idea of a "natural" rate which would equilibrate the system at full employment is just a corollary to rejecting the full employment assumptions built into Say's Law. By his analysis of the social role of money as the liquidity standard, Keynes has shown that money as a social institution (whether a free-money or a state-money) has important (negative) externality effects. Harrod's comment about a "wrong" rate of interest that is"natural, durable, and in a certain sense, in the free system inevitable," can now be seen as an insight stemming directly from Keynes' essential properties of interest and money.

By channeling the richness of Keynes' vision of the financial aspects of a modern money economy into a framework which identifies these equilibriums, the own-rates theory provides another angle on Keynes' theoretical revolution. And from the standpoint of further work in macroeconomics, the lesson of this view is that trying to recast Keynes into a Wicksellian world is a graft that will not take. Keynes' monetary theory is not a simple afterthought, or appendage that can easily be thrown overboard to pursue orthodox concerns. That is, not unless we are willing to revert to a full-employment paradigm where unemployment can

only arise from a friction or rigidity, incompatibly thrust into an otherwise smoothly operating system. This is the error we have seen that led the "Keynesian Revolution" to the impasse it faces today. Such a full employment framework is just the sort of propaedeutic world that Keynes was warning us against when he wrote:

> Or we can pass from this simplified propaedeutic to the problems of the real world in which our previous expectations are liable to disappointment and expectations concerning the future affect what we do today. It is when we have made this transition that the peculiar properties of money as a link between the present and the future must enter into our calculations. But, although the theory of shifting equilibrium must necessarily be pursued in terms of a monetary economy, it remains a theory of value and distribution and not a separate "theory of money." Money in its significant attributes is, above all, a subtle device for linking the present to the future; and we cannot even begin to discuss the effects of changing expectations on current activities except in monetary terms. We cannot get rid of money even by abolishing gold and silver and legal instruments. So long as there exists any durable asset, it is capable of possessing monetary attributes and, therefore, of giving rise to the characteristic problems of a monetary economy (1936, pp. 293-294).

VI. ENDNOTES

1. "If I were writing again, I should indeed feel disposed to define full employment as being reached at the same moment at which the supply of output in general becomes inelastic," Keynes to J. R. Hicks, August 1936 (Keynes, 1973b, p. 71). For a discussion of the analytical implications of this view of Keynes (a view that the present essay is in complete agreement with), see Darity and Horn (1983).

2. Keynes himself traces his views on non-calculable uncertainty and the necessary element of confidence in such judgments of the future to his early work as a probability theorist (Keynes, 1921). See Keynes (1936, pp. 148-149 and p. 240) for the link with his interest rate theory.

3. This Full Information/Full Employment benchmark seems to have an enduring place in Austrian theory. See Mises (1966, pp. 244-252) for an account of the "Evenly Rotating Economy" as the foundation of the aprioristic science of "Cattalactics." O'Driscoll (1977, pp. 68-70) defends Hayek's use of such a benchmark case. In Essay III, we will compare this concept with Keynes' "shifting equilibrium" in more detail.

4. We will quote from the now generally used second edition of Prices and Production first published in 1935. Sraffa's critique, of course, was of the original 1931 edition. But a side-by-side perusal of these two editions reveals no substantive change beyond the addition of a few extra footnotes. It would seem that Hayek did not find Sraffa's criticism convincing enough to change his argument when the opportunity

arose; although see Hayek (1941, p. 35, n. 1) in this connection.

5. "Hayek constructed his monetary theory upon the foundations laid by early British monetary theorists and Knut Wicksell and Ludwig von Mises" (O'Driscoll, 1977, p. 37).

6. According to Marshall's most faithful student and hand-picked successor, A. C. Pigou (1925, p. 84), Marshall's method led him away from formalistic theory:

> Though a skilled mathematician, he used mathematics sparingly. He saw that excessive reliance on this instrument might lead us astray in pursuit of intellectual toys, imaginary problems not conforming to the conditions of real life; and further, might distort our sense of proportion by causing us to neglect factors that could not easily be worked up in the mathematical machine.

7. In Essay III, where we analyze Hayek's defense against Sraffa's critique, we will argue that Hayek eventually does abandon the Wicksellian scheme.

8. Later, Keynes would also claim a relationship between his interest rate theory and Fisher's: "I find, looking back, that it was Professor Irving Fisher who was the great-grandparent who first influenced me strongly toward regarding money as a 'real' factor" (1937b, p. 203n).

9. In fact, many interpreters of Keynes' Theory, both hostile and sympathetic, have found the argument of chapter 17 to be not only highly abstract--but unintelligible. For instance, the man generally credited with "bringing Keynes to America," Alvin Hansen, had this to say in his famous Guide to Keynes (1953, p. 159):

> Chapter 17, on the properties of interest and money, ties in with the subject matter of money and liquidity preference. . . . But the

> topic is elevated to a very abstract plane. Immediately after the appearance of the General Theory there was a certain fascination about Chap. 17, due partly no doubt to its obscurity. Digging in this area, however, soon ceased after it was found that the chapter contained no gold mines . . . in general, not much would have been lost had it never been written.

With expositors like this, it is no wonder Keynes' monetary views suffered such eclipse. This attitude carried over to Hansen's most famous student, P. A. Samuelson, who also must be credited with a large part of the "Keynesian" version of Keynes. In an evaluation of Keynes as an economist (1947b) in which Samuelson relegates the own-rates theory to the category of "Mares' nests or confusions" (p. 149), and in which he explains why "liquidity preference . . . cannot be of crucial significance," he draws the following remarkable conclusion about Keynes as an economic theorist:

> . . . Keynes seems never to have had any genuine interest in the theory of value and distribution. It is remarkable that so active a brain would have failed to make any contribution to economic theory (p. 155).

One supposes this view of Keynes' contribution goes a long way toward explaining the easy cooptation of "Keynesianism" by orthodox neoclassical theory.

Turning to hostile reviews, one that takes a particularly venomous line on chapter 17 is H. Hazlitt's (1959) "The Failure of the 'New Economics'." Hazlitt's volume has much to recommend itself as a curative to anyone considering putting a vituperative attack into print. It reserved a large quota of its venom (and exclamation points!) for Chapter 17:

> Chapter 17 of the General Theory, "The Essential Properties of Interest and Money," is dull, implausible, and full of obscurities, non sequitors, and other fallacies (p. 236).
>
> Of all of the confusions in the General Theory, this is one of the most incredible (p. 237).

10. The role of Keynes' intricate analysis of the circulatory route of cash in different uses in the economy was laid out in the Treatise (1930, book 1, ch. 3). Although it is suppressed in the General Theory, this analysis is essential to a complete understanding of liquidity preference theory. See Shackle (1967, ch. 15) for an insightful blending of the two strands of the theory of cash balances.

11. This intricate fusion of capital theory and monetary theory is the core of Shackle's (1967, ch. 11) unique blending of Keynes' views on money and uncertainty in the General Theory with his crucial restatement in 1937c. Shackle makes no use of the own-rates paradigm, but I think our discussion to this point might clarify his argument for some readers. For instance, the following eloquent passage nicely complements our view with that grace of expression that is Shackle's hallmark:

> Writers on Keynes's theory of investment incentive give all their attention to the concepts of the marginal efficiency of capital and the interaction of a quantity so named with the interest-rate on loans of money. To do so is to study the formal configuration of the engine without asking about its thermal source of power. The marginal efficiency of capital is nothing but a formal sum waiting for the insertion of numerical values in place of its algebraic symbols. The essential problem of why at any time the investment flow has the size it has is contained in the question what is the source of these numerical values, by which psychic alchemy is the list of incongruous ingredients chosen and fused into an answer to the unanswerable. Keynes's whole theory of unemployment is ultimately the simple statement that, rational expectation being unattainable, we substitute for it first one and then another kind of irrational expectation: and the shift from one arbitrary basis to another gives us from time to time a moment of truth, when our

> artificial confidence is for the time being dissolved, and we, as business men, are afraid to invest, and so fail to provide enough demand to match our society's desire to produce. Keynes in the General Theory attempted a rational theory of a field of conduct which by the nature of its terms could be only semi-rational. But sober economists gravely upholding a faith in the calculability of human affairs could not bring themselves to acknowledge that this could be his purpose. They sought to interpret the General Theory as just one more manual of political arithmetic. In so far as it failed this test, they found it wrong, or obscure, or perverse. The same fate had overtaken his Treatise on Probability (1967, p. 129).

12. In fact, Irving Fisher found an actual example of the effect on interest rates that uncertainty, not just over the stability of the monetary unit, but uncertainty over its actual existence has. He analyzes an example in his The Rate of Interest (1907, pp. 258-261) where two types of U.S. bonds existed simultaneously that were payable in gold and paper notes, respectively. The variations in the spread between the two over the years 1870 to 1896 are attributed by Fisher to the changing expectations of the future form of the standard of payment caused by the then active public debate over the various proposals of the "money doctors."

13. For a complete analysis of the irrelevance of fixed money-wages to Keynes' definition of unemployment equilibriums, see Darity and Horn (1983).

14. Keynes seems to have thought that most of the adjustment in the "a" terms would be accomplished by movements in the present spot prices. See the discussion in chapter 17 (p. 228) of an example of the equilibration of own-rates. Also, in a protracted pre-publication correspondence with R. C. Hawtrey, Keynes defends his own-rate theory in the case of stocks of liquid commodities from Hawtrey's criticism (on

both theoretical and practical grounds). In the process, he makes the same point about the adjustment factor "normally" being the spot price (1973a, p. 629).

15. In Keynes' collected works (1979, p. 235), Townshend is identified as follows: "Another discussant was Hugh Townshend (1890-1974), who, after taking a first in mathematics in Cambridge in 1912, had been a pupil of Keynes while preparing for Civil Service examinations in 1914. He had then entered the Post Office." Besides the note referred to in the text and a few scattered reviews in the Economic Journal in the late '30s, the only other work of his I have found is a co-authored book (Curtis and Townshend, 1938). This book is interesting in that it is an attempt to provide a layman's guide to the workings of a "monetary economy" via the views of two authors who "belong to the school of thought associated with the name of J. M. Keynes" (p. vi). That this self-identification is not idle (at least for Townshend) is attested to by Keynes' surviving correspondence with Townshend in which he credits him with even more than a complete understanding of his theory. For example, consider the following fragments from Keynes' letters to Townshend (in Keynes, 1979, pp. 235-247 and 255-259):

> It is evident that you have a perfect comprehension of the matter; and indeed it may prove to be the case that, whilst the book is chiefly meant for my academic friends, it may sometimes get easier reception from those outside academic circles, whose ideas are not so crystallised (p. 238).
>
> Once more you have shown a complete comprehension of what I am driving at, and I am very grateful (p. 239).

> Once again I have to thank you for an acute and understanding criticism, with the whole of which, I think I may say, I agree (p. 245).
>
> Criticisms like yours are mainly useful in helping me to get more fully emancipated from what one has emerged out of (p. 247).

This last fragment reinforces the view of Shackle that Townshend saw earlier and deeper than most writers into the depths of the departure from orthodoxy that Keynes' views on money and interest represent:

> Townshend's brilliant paper, although thirteen pages long, appeared only under Notes and Memoranda. It leapt too far ahead for the mass of Keynes's critics, still tapping the wheels of his theory to see whether it would clank decently round like the sort of thing they were used to, and Townshend attracted no attention (1967, p. 228).

16.

> Moreover, it would seem to follow that there can be no such thing as long-period dynamic economic theory, failing the (most unlikely) discovery of a plausible long-term convention of price-stability. It is perhaps now being generally realised that such long-term dynamic theories as these are considered unplausible ones. It is not unnatural that those who forecast the future in algebra or geometry should be chastened by hard fact more slowly than those who have to forecast it in arithmetic. Nor is the conclusion that the search for laws to enable us to predict economic events far ahead, like eclipses, must be given up, so surprising--not to say nihilistic--as it may seem (to some economists) at first sight. For in the past, in long periods prices have in fact moved all over the place. The inference that there is no reason to believe in the probable indefinite recurrence of a <u>regular</u> cycle of price-fluctuations is less generally accepted, but seems to follow from Mr. Keynes' conclusions (Townshend, 1937, p. 166).

17. There is an interesting parallel between Keynes' discussion of a liquidity standard in a nonmonetary economy and Menger's (1892; 1976, ch. 8) hypothetico-historical account of the origin of a medium of exchange. Both revolve around the concept of "marketability." I think a comparison of them would show a strong complementarity between the discussions. Menger explains the process and reasons for the evolution

of a generally accepted means of payment. Keynes shows that this spontaneously evolved social institution has important externality effects--even in the most primitive monetary systems.

18.

> The liquidity premium, it will be observed, is partly similar to the risk-premium, but partly different;--the difference corresponding to the difference between the best estimates we can make of probabilities and the confidence with which we can make them (Keynes, 1936, p. 240).

ESSAY III. THE SRAFFA-HAYEK DEBATE IN ANALYTICAL PERSPECTIVE

I. INTRODUCTION

The previous essay attempts to show the extent to which Keynes' views on money and interest can be given analytical focus by situating them in the own-rates of interest framework developed in Chapter 17 of the General Theory. When analyzed in terms of the shifting equilibrium on the asset market suggested by Keynes' use of the own-rates theory, both the possibility of unemployment equilibriums and his unique approach to expectations, uncertainty, and money can be seen as part of the same theoretical picture. Having defined this approach in terms of unemployment equilibriums, it is natural to ask how, or if, the own-rates equilibrium approach differs from orthodox equilibrium approaches. In the present case, this is an especially compelling exercise for two interrelated reasons.

First, in terms of the historical record, the immediate response to Sraffa's original conception of commodity own-rates, put forth in his review of Hayek (Sraffa, 1932a), took the form of Hayek claiming that his concept of equilibrium included the simultaneous existence of many commodity rates. Since we have argued that Sraffa's commodity-rate concept represents a formidable attack on the whole Austro-Wicksellian business cycle theory based on the conception of a "natural" rate of interest, Hayek's defense of his position warrants attention. Moreover, this historical debate is linked explicitly with our second, and more modern, reason for investigating the equilibrium conception behind the own-rates framework. Recently, the neo-Walrasian conception of a full

intertemporal equilibrium model has been traced to Hayek's own work in the '20s and '30s (Milgate, 1982, pp. 125-142). Since this modern general equilibrium framework also yields a complete set of commodity specific interest rates ancillary to the equilibrium price vector, it presents prima facie parallels with the own-rates framework. Once again, the synergism of the history of economics and modern developments is an important theme.

Following out this synergistic relationship, the present essay will review the Sraffa-Hayek debate from the standpoint of the light it might shed on the question of alternative equilibrium frameworks for discussing money and interest. Our conclusion will be that there are (at least) two distinct approaches involved here, but that recourse to the intertemporal equilibrium construct to dodge Sraffa's criticism involves Hayek in a substantial abandonment of his original Wicksellian position. This conclusion is reinforced by the later development of Hayek's monetary thought away from equilibrium concerns and toward institutional process analysis of the social functions of money.

Pushing forward to the modern conception of Walrasian intertemporal equilibrium, we will compare this most general of frameworks with Keynes' own-rates theory. Here, our conclusion will be that the neo-Walrasian approach fails to address the basic social and institutional motivations upon which Keynes based his approach to a monetary theory of production.

Essentially our argument will be that the neo-Walrasian approach shares with the traditional neoclassical long-period theory (e.g., as represented by Wicksell) a barter stance toward value determination that

logically excludes the most interesting qualities of a monetary economy, while throwing out the corresponding long-period vision of competition. This argument will be brought out by a comparison of Sraffa's long-period use of own-rates in his critique of the Wicksellian framework with the own-rates that come out of a neo-Walrasian intertemporal equilibrium. Having gained this insight, we can view Keynes' own-rates equilibrium framework as an intermediate case between Sraffa's and the neo-Walrasian's conception of equilibrium own-rates. From this standpoint, Keynes' shifting equilibrium maintains the long-period concern with the tendency of competition to equalize rates of return, while providing a short-period financial mechanism by which the monetary elements of conventional judgment, expectation, and liquidity effect the level at which this rate settles. A quick review of Sraffa's and Keynes' use of own-rates will set the stage for our discussion.

II. SRAFFA AND KEYNES' USE OF OWN-RATES

We have already laid out the details of Sraffa's (1932a) critique of the Austro-Wicksellian natural rate framework in Essay II. Briefly, Sraffa's criticism can be summed up in two main points. In his analysis of the Wicksellian natural rate concept, Sraffa made the important point that in a barter-like state,

> there would be a single rate which satisfies the conditions of equilibrium, but there might at any moment, be as many 'natural' rates of interest as there are commodities, though they would not be 'equilibrium' rates (1932a, p. 45).

Thus, a unique "natural rate," such as the Wicksellian monetary framework revolves around, cannot be defined. Secondly, Sraffa finds that because these divergent commodity rates are common to any exchange economy, monetary as well as barter, where the conditions of long period equilibrium are not fulfilled, the Wicksellian dichotomy between a nonmonetary, "real" natural-rate, and a bank-influenced "money" rate is a flawed approach to distinguishing the monetary and real influences in economic theory. Instead, Sraffa suggests, the differences between a monetary and nonmonetary economy should be sought in

> those characteristics which are set forth at the beginning of every textbook on money. That is to say, that money is not only the medium of exchange, but also a store of value, and the standard in terms of which debts, and other legal obligations, habits, opinions, conventions, in short all kinds of relations between men, are more or less rigidly fixed (1932a, p. 43).

We have also shown that it was exactly these characteristics of money as a public good that Keynes amplified into a complex "shifting equilibrium" framework of production, asset holding, and relative prices.

Before comparing Sraffa's views with Hayek's, it is important to emphasize one additional point from our previous discussion. That is that the concept of equilibrium own-rates differs for Sraffa and Keynes. For Sraffa, recall, the equilibrium was defined by the traditional long-period position of equal value returns in all production sectors. In this context, he defined equilibrium own-rates as the situation where spot and future prices coincide and where all the different commodity rates and the money rate take on the same value (1932a, pp. 50-51).

Keynes' use of the own-rates implied a very different conception of the equilibrium levels. For him, arbitrage in the asset market would drive the spot and future prices for existing assets into a configuration (not equality) where all own-rates, defined in money terms, offered equal financial rates of return to the marginal investors. Thus, in his system, where asset markets are continuously equilibrated by arbitraging, the importance of the equilibrium is as a centering device for the interaction of the influences of expectation, social convention and money on the one side, with the productive flows of investment activity on the other. As we argued above, the difference between Sraffa's and Keynes' use of the own-rates can be set down as an example of the difference between Keynes' shifting equilibrium ("the theory of a system in which changing views about the future are capable of influencing the present situation"), and the theory of stationary equilibrium (where "we can consider what distribution of resources between different uses will be consistent with equilibrium under the influence of normal economic motives in a world in which our views concerning the future are fixed and

reliable in all respects") (Keynes, 1936, p. 293).

It will be noticed that such a conception as Keynes drew out of Sraffa's commodity rates addresses exactly those textbook characteristics of money and conventional wealth-relations that Sraffa hinted were being lost sight of in the Wicksellian framework. From this standpoint, the use of own-rates in the review of Hayek can be viewed as immanent criticism of the orthodox Marginalist approach to monetary theory (of which the Austro-Wicksellian variant was a prime example). Alternatively, Keynes' elaborate framework of Chapter 17 can be seen as a constructive alternative to that traditional theory. This distinction will aid our understanding of the tortuous twists and turns that following out Hayek's defense of his position will lead us to. To that end, it will be useful to draw this distinction out a bit more thoroughly.

As we have mentioned (and as Sraffa's discussion makes clear[1]), Sraffa's use of the concept of own-rates is basically a classical long-period conception. In this conception, each productive sector yields an equal rate of return, and all prices are at their long-period normal levels. Accordingly, his definition of equilibrium own-rates, where the spot and future price of each good coincide, and where each own-rate equals any other and the money-rate, is also a long-period conception. We have also shown that Keynes explicitly distinguishes his "shifting equilibrium," defined in Chapter 17 in own-rate terms, from any "stationary equilibrium" approach to value theory (such as Sraffa's equilibrium own-rates would imply) by the use of expectations made in

each. Thus in Keynes' framework, "equilibrium" own-rates do not imply the equality of all spot and future prices as in Sraffa's conception. Instead, he brings in expectations and so drives his own-rates into equality today, based on uncertain judgments of prices in a never-realized future.

It is instructive to note that Keynes makes this expectations-based distinction between the theory of shifting equilibrium and the theory of stationary equilibrium, in the context of a critique of the traditional separation of the theory of value and the theory of money (1936, pp. 292-294). He complains of the approach by which relative prices are determined by supply and demand in discussions of the theory of value, but money prices are grounded in a completely different approach, usually some variant of the quantity theory. Keynes claims that one of his objects in the General Theory is

> . . . to escape this double life and to bring the theory of prices as a whole back to close contact with the theory of value (p. 293).

Given that we have equated the own-rates framework with this attempt by Keynes to blend value theory and monetary theory, it is useful to ask how it bridges the dichotomy he found in the traditional approach. The uniqueness of this approach when compared to the traditional long-period specification of equal returns on capital in all sectors (whether this is a constant no growth stationary state, or just a steadily expanding long-period as in Marshall's more dynamic conception[2]) concerns two points.

First, Keynes' shifting equilibrium relies on revaluation of existing assets (both real and financial) on the second-hand markets to

continuously ensure equal "financial," or money-denominated, returns. This is the specification illustrated by the (arbitrage-driven) relation between all consistently measured own-rates of interest in equilibrium. What distinguishes this approach from the traditional marginalist theory of long-period equilibrium is that Keynes explicitly includes both financial and real aspects of investment opportunities in his conception of the rate-of-return-equalizing function of competition. This is one aspect of his distinction between the theory of shifting equilibrium and the theory of stationary equilibrium.

Secondly, this distinction involves the generality that Keynes claimed for his interest theory over that of the classicals. As he explicitly stated in his discussion of "The Classical Theory of The Rate of Interest" in Chapter 14 and its appendix, the obverse side of the failure to recognize less-than-full-employment equilibrium incomes is an incomplete theory of the rate of interest:

> Thus the functions used by the classical theory, namely the response of investment and the response of the amount saved out of a given income to change in the rate of interest, do not furnish material for a theory of the rate of interest; but they could be used to tell us what the level of income will be, (given from some other source) the rate of interest; and alternatively, what the rate of interest will have to be, if the level of income is to be maintained at a given figure (e.g. the level corresponding to full employment) (1936, pp. 181-182).

What was missing in the classicals was the recognition of the link between changing income and the interest rate via the rate of investment. Keynes, by giving the monetary aspects of investment behavior equal footing with the real aspects, was able to provide this link. The own-rates equilibrium provides the most general form of his argument on this

topic. When financial and capital assets are each equally attractive investment opportunities, the own-rates of each sector will be driven to equality by wealth holders. But what is more important is that the level of interest set in this asset-market equilibrium governs the flows of new investment goods production, and so affects aggregate demand. Thus follows the importance of money and liquidity preference if these monetary influences, for fundamental reasons of the economic environment, set the pace of investment activity. Unlike the classical system which could only define the equilibrium configuration of interest rates (i.e., where the interest rate equals the marginal efficiency of capital), Keynes also provides a theory of the level at which this configuration will settle.

In terms of the place of the own-rates theory in both Sraffa's imminent criticism of the orthodox approach and Keynes' constructive blending of monetary and value theory, the fundamental question is the choice of a method by which monetary influences are allowed to enter economic theory. Keynes clearly thought that it obscured the powerful role of monetary factors to relegate them to a disturbing cause that only enters as an afterthought to an otherwise purely "real" theory of relative prices. He made this point as early as 1933 in his contribution to the Speithoff festschrift, "A Monetary Theory of Production"; he repeated it in the General Theory and continued to emphasize it in his review of his critics in his post-General Theory defense (1937a,b,c). Not surprisingly, then, this same fundamental point turns out to be the central focus of the Sraffa-Hayek debate from which Keynes lifted the

own-rate approach in 1932. With a view to Keynes' place in relation to the tradition of treating monetary factors as a deviation from long-period equilibrium, we now turn to Hayek's reply to Sraffa on these issues.

III. HAYEK'S DEFENSE

Hayek clearly recognized the fundamental nature of Sraffa's critique. Accordingly, his reply notes that both he and Sraffa are interested in the answer to the methodological question as to "where the essential differences between a monetary and a non-monetary economy are to be sought" (Hayek, 1932b, p. 238). In 1932, Hayek felt that this was an "obvious matter" and he states his view quite clearly:

> I have been assuming that the body of existing pure economic theory demonstrates, that so long as we neglect monetary factors, there is an inherent tendency toward an equilibrium of the economic system; and what I tried to do in Prices and Production, and in certain earlier publications, was to show that monetary factors may bring about a kind of disequilibrium in the economic system--which could not be explained without recourse to these monetary factors (p. 232).

Here, Hayek has succinctly summed up the traditional treatment of monetary factors in the value theory of the 1930s. In so doing, he clearly identifies his natural rate approach with the orthodoxy Keynes was dissenting from. We have already touched on the mechanics of the Austro-Wicksellian version of this tradition in our discussion of Hayek's business cycle theory above (Essay II, pp. 96-99). What is central to the present case is the idea that without bank-controlled money, which makes possible the altering of the market rate away from the natural rate, that the real economic system would tend toward a full-employment, long-period equilibrium (see Hayek, 1935, pp. 30-31; Wicksell, 1935, pp. 159-168; Leijonhufvud, 1981, pp. 131-202). This much of Hayek's reply is not surprising and on its basis alone we could attribute to Hayek his earlier professed reliance (1935, pp. 22-25) on the relative price theory

(if not the monetary price-level theory) embodied in the Wicksellian framework which he described in Prices and Production:

> Put concisely, Wicksell's theory is as follows: If it were not for monetary disturbances, the rate of interest would be determined so as to equalize the demand for and the supply of savings. This equilibrium rate, as I prefer to call it, he christens the natural rate of interest. In a money economy, the actual or money rate of interest ("Geldzins") may differ from the equilibrium or natural rate, because the demand for and the supply of capital do not meet in their natural form but in the form of money, the quantity of which available for capital purposes may be arbitrarily changed by the banks (p. 23).

Note that this quote includes the passage Sraffa had picked out in focusing his criticism on the conception of a unique natural rate in an accumulating economy. His simple-minded objection was that such a barter-like natural rate would be impossible to define within this framework. Given this context of agreement with the Wicksellian theory, then, what is surprising in Hayek's reply is that he bases his objection to Sraffa's strictures on the argument that he had all along been aware of the simultaneous existence of a multiplicity of commodity rates. Referring explicitly to Sraffa's contention that if money did not exist, "there might, at any moment, be as many 'natural' rates of interest as there are commodities," Hayek replies:

> I think it would be truer to say that, in this situation, there would be no single rate which, applied to all commodities, would satisfy the conditions of equilibrium rates, but there might, at any moment, be as many "natural" rates of interest as there are commodities, all of which would be equilibrium rates (1932b, p. 245).

Clearly what we have here is a difference of opinion over the precise meaning given to the term equilibrium. At least two conceptions can be distinguished so far. First, we have Sraffa's usage, already

defined as a position where each productive sector yields an equal rate of return, or where the money rate and all commodity rates are the same. As we have mentioned (and as Sraffa's discussion makes clear), this is basically a classical long-period conception. Sraffa's use of the long-period conception in an immanent criticism of the Wicksellian framework is in full accordance with the whole tradition of post-marginal-revolution monetary theory to which we compared Keynes above. P. A. Samuelson (1968, pp. 170-171), in a look back to what classical and neoclassical monetary theory "really was" before Keynes, (or as he puts it, what a "jackass" neoclassical monetary theorist believed before 1937), succinctly sums up this position:

> Essentially, we believed that in the longest run and in ideal models the amount of money did not matter. Money could be "neutral" and in many conditions the hypothesis that it was could provide a good first or last approximation to the facts. To be sure, Hume, Fisher and Hawtrey had taught us that, under dynamic conditions, an increase in money might lead to "money illusion" and might cause substantive changes. . . . But all of this was at a second level of approximation, representing relatively transient abberations. Moreover, this tended to be taught in applied courses on business cycles, money and finance and economic history rather than in courses on pure theory. In a real sense there was a dichotomy in our minds; we were schizophrenics.

This schizophrenic treatment of money as an afterthought to the "real" determination of relative prices was the foundation upon which the monetary theories of Wicksell and the other post-revolution Marginalists were built. Keynes mentions Marshall, Bohm-Bawerk, and Carver (among others) in this connection (1936, pp. 175-193), as examples of the classical interest rate theorists. He distinguishes as "neo-classical" those monetary theorists who still maintained the theoretical distinction

between prices and money, but who "attempt to build a bridge" between the two. It is with this group that he places the concept of a "natural rate of interest" and the idea of forced savings that play so much a part in the Austro-Wicksellian scheme.

Thus, by most standards, it seems appropriate to group Hayek's argument in Prices and Production within the neoclassical tradition of investigating the short-run disturbances by which monetary influences cause deviations from a traditional long-period equilibrium. Surely this is what Hayek was referring to in his statement above about the "inherent tendency toward an equilibrium" of the system in the absence of monetary factors and his identification of monetary theory with "a kind of disequilibrium in the economic system." This perception is further reinforced when we see Hayek rebuking Sraffa for misunderstanding the context of his monetary theory:

> I do not quite understand whether Mr. Sraffa thinks that, in order to show this [the disturbing influence of monetary factors], it would have been necessary first to re-state the whole of equilibrium economics. I thought this was not only impossible within the limits of a small book, but also quite unnecessary (Hayek, 1932b, p. 238).

In fact, as Sraffa's critique and Keynes' development of the own-rates theory of interest now make clear, this is exactly what Sraffa was advocating in 1932. But, realizing Hayek's strong identification with the Wicksellian tradition, this makes his further comment about the existence of a multiplicity of divergent, but nevertheless "equilibrium," rates of interest all the more perplexing. For this second conception of equilibrium, the theory of intertemporal equilibrium is in direct opposition to the traditional long-period method by which Wicksell (and

Hayek in Prices and Production) determined the natural rate of interest to which the market rate was compared in analyzing the short-run influence of money.

We will investigate the details of this modern conception of capital and interest in the next section. For our present purposes, it is sufficient to point out its use by Hayek and its contradiction of the natural rate framework that he seems to have simultaneously held. The link with our present historical investigation is provided by Murray Milgate's recent Capital and Employment (1982), where he identifies Hayek as the originator of the conception of an "intertemporal equilibrium." Essentially, the conception of intertemporal equilibriums involves a logical extension of the Walrasian general equilibrium model to a situation where goods are distinguished by dates of availability, as well as their other characteristics. In this context, the same good at different dates might have different prices. The relationship between the same good's price at different dates forms the implicit interest rate on each good, none of which logically need be equal (see Bliss, 1975, Ch. 3).

The crucial difference between this conception and the traditional long-period equilibrium method is that since all prices, current and future, are determined at once, and only yield ancillary interest rates that are implicit in the equilibrium price vector, no equalized rates of return in different productive sectors is called for. Indeed, this foundation-stone of the traditional approach to value theory by which a uniform general rate of return (profit) (e.g. Wicksell's natural rate) is

the object of analysis, is completely lost in the intertemporal equilibrium framework. Milgate (1982, p. 136) notes this radical change in the notion of equilibrium.

> To arrive at the notion of intertemporal equilibrium, one has first to cut loose the short-period problem from its traditional long-period moorings (which amounts to a severing of the traditional conception of "equilibrium" and "disequilibrium") and then to install it into the centre of the picture. This, of course, is precisely the point at which the early work of Lindahl and Myrdal is so strikingly at variance with that of Wicksell.

Milgate documents the existence of this conception ("so strikingly at variance with that of Wicksell") in Hayek's early monetary theory. He shows that the idea of dating commodities is present in Prices and Production and that the notion of an intertemporal equilibrium appears clearly in Hayek's Pure Theory of Capital (1941). Historically, the concept seems to have originated in a 1928 paper by Hayek which has just recently been translated as "Intertemporal Price Equilibrium and Movements in The Value of Money" (1928). This early paper is particularly interesting to our theme since it both explicitly identifies Hayek as an early expositor of intertemporal equilibrium theory, and contains his earliest views on the place of money in equilibrium value theory.

With regard to the relation of monetary theory and price theory, in 1928 Hayek is adamant that money should be viewed as falling within the purview of equilibrium theory. To this end, he faults the traditional static equilibrium theory for neglecting the influence of time on the production and consumption of goods.

> As soon as these assumptions [of static theory], oversimplified and all too contradictory of reality as they are, are replaced by ones corresponding more to the facts, it becomes evident that the customary abstraction from time does a degree of violence to the actual state of affairs which casts serious doubt upon the utility of the results thereby achieved (pp. 71-72).

Hayek's view of the value of equilibrium theory at this point is also clearly stated.

> Yet the concept of equilibrium is just as indispensible a tool for the analysis of temporal differences in prices as it is for any other investigation in economic theory. Strictly speaking, its field of application is identical with that of economic theory (p. 75).

So if time has been neglected, but equilibrium is indispensible, then an extension of static value theory that will adequately account for the influence of time and money within the traditional theory of equilibrium states is called for:

> From the moment at which the analysis is no longer concerned exclusively with prices which are (presumed to be) simultaneously set, as in the elementary presentations of pure theory, but goes on to a consideration of the monetary economy, with prices which necessarily are set at successive points in time, a problem arises for whose solution it is vain to seek in the existing corpus of economic theory. Instead of needing to explain merely the necessity for the existence of a particular structure [Abstufung] of simultaneously existing prices and its function, what must now be done is to analyze the necessity and significance of relative levels of prices at successive points in time.

It is in pursuit of this goal that Hayek introduces the idea of an "intertemporal price system" which would determine intertemporal exchange ratios over time between goods, all of which would be equilibrium rates. Though the style of this paper, translated from the academic German of the 1920s, is tortuously turgid, it provides many historically interesting points. These include a discussion of the definition of the

elementary time period for which intertemporal relations will be defined, the "data" that are analytically necessary to define such an equilibrium, and the meaning of the divergent commodity rates so defined. In particular, Hayek's treatment is very suggestive of the form that Hicks' "temporary equilibrium" model would take in 1939 (Hicks dates his "personal revolution" in economic theory from his association with Hayek at the L.S.E. in the thirties[3]). But for our purposes in trying to place Hayek's approach to monetary theory in the context of the theoretical scene of the 1930s, this flirtation with intertemporal equilibrium seems only to add perplexity to confusion.

What is perplexing is the fact of Hayek's continual shifting of his position in the monetary debates of the period--and often to completely self-contradictory analytical positions. Of this analytical hopscotch, the reply to Sraffa is just one jump. Sraffa, recall, had explicitly aimed his criticism of the barter-like natural rate toward the Wicksellian framework that Hayek claimed to be utilizing in Prices and Production. Having shown that this concept could not be uniquely defined, Sraffa implied it mattered little since such an attempt to separate value theory from the real concerns of a monetary economy invalidated it from the start. In reply to this, then, Hayek is professing to hold onto the traditional long-period separation between relative prices and money, and simultaneously embrace an "intertemporal equilibrium" that invalidates the very conception of the Wicksellian natural rate.

Hayek seems to have been thrown off balance by Sraffa's critique, which is not surprising when we recall the theoretical turmoil of the period. In the end, though, Hayek cannot have it both ways. As Sraffa's rejoinder makes clear, the choice between an intertemporal equilibrium approach and the natural rate framework is a choice between two mutually exclusive theoretical positions:

> Dr. Hayek now acknowledges the multiplicity of the "natural" rates, but he has nothing more to say on this specific point than that they "all would be equilibrium rates." The only meaning (if it be a meaning) I can attach to this point is that his maxim of policy now requires that the money rate should be equal to all these divergent natural rates (1932b, p. 251).

In historical terms, Hayek can hardly be faulted for his confusion over the precise meanings of theoretical developments that were being invented as he wrote. In fact, the distinction about the different notions of equilibrium that lie behind various theoretical systems is a development of a fairly recent vintage (see Hahn, 1973; Garegnani, 1976; Milgate, 1982)[4]. Moreover, he was in the good company of Wicksell, Marshall and even Ricardo in having been shown a subtle and implicit assumption of his own system by that piercing gadfly Piero Sraffa. But what is perhaps less to Hayek's credit is his practice of freely shifting his methodological ground when his theory met with criticism (while suspiciously deriving the same policy conclusions from each alternative approach). We have seen one instance of this in the Sraffa-Hayek debate. A quick survey of the latter-day development of his methodological views and his views on money reveal a disturbing continuance of this pattern.

In terms of his methodological position toward equilibrium economic theory, Hayek's views have run the gamut from zealous advocacy to complete rejection. T. W. Hutchison (1981) has recently surveyed Hayek's methodological work in economics and concluded that we need to distinguish at least two Hayeks:

> Through the multifarious experiences and upheavals of the middle decades of the twentieth century, Hayek's long intellectual career has shown much constancy of view. . . . But, on some quite fundamental and very important points of methodology and philosophy, vital and critical changes in Hayek's views can be discerned--as also, incidentally, on money and on some issues of employment policy--which have not received the attention and appreciation which they deserve (pp. 210-211).

It would be far beyond our present purposes to provide a comprehensive survey of the development of Hayek's multifarious views on the method of the social sciences (see Hutchison, 1981). What is sufficient to our case is to point out that Hayek's ambivalent position on these matters is not isolated to the debate with Sraffa and that his shifting methodological views parallel a shifting view of the importance and place of money in economic theory.

As we have seen, Hayek in the 1920s and '30s seems to have been simultaneously a Wicksellian natural-rate-theorist, looking to monetary <u>disequilibriums</u> to define what happens "in the first place" (Wicksell, 1935, p. 159) when a long-period relative-price equilibrium is upset, and also a Walrasian[5] "intertemporal" equilibrium theorist trying to define a general <u>equilibrium</u> treatment of money as a set of prices over time. Neither of these approaches, of course, dealt with any of those characteristics of money as a social institution grounded in an uncertain

environment that were the hallmarks of Keynes' treatment. In fact, Hutchinson argues that the early Hayek, following his mentor Mises, held a commitment to economic equilibriums so ardent that he considered it a stronger conception than the corresponding idea in the natural sciences. Hutchinson quotes the following in this context:

> The essential basic facts which we need for the explanation of social phenomena are part of common experience, part of the stuff of our thinking. In the social sciences it is the elements of the complex phenomena which are known beyond the possibility of dispute. In the natural sciences they can at best be surmised (Hayek, 1949, p. 126, italics added).

Yet, after the General Theory was published, we see Hayek turning away from his previous commitments to mechanistic equilibrium theory and castigating economists for trying to ape the "scientistic" methods of the natural sciences (Hayek, 1952, pp. 14-16). This further evolution in Hayek's thought, away from either an equilibrium or disequilibrium approach, toward a legalistic, institutional process analysis of social phenomena, began with his famous "Economics and Knowledge" in 1937 (discussed in detail in Essay I, above). Here, we find Hayek critiquing the whole idea of formal equilibrium states (which, recall, had been "identical with economic theory" in 1928) as little more than a tautologous starting point of analysis. To see the extent to which his position had changed from 1932 to 1937, compare the following statement with Hayek's rebuke to Sraffa about the "obvious" conclusion of existing equilibrium theory (quoted above, pp. 189 and 192).

> I am afraid that I am now getting to a state where it becomes exceedingly difficult to say what exactly are the assumptions on the basis of which we assert that there will be a tendency toward

> equilibrium and to claim that our analysis has an application to the real world (1937, p. 60).

It is from this critique of equilibrium theory that Hutchison dates the birth of Hayek II (we might say Hayek III, given our knowledge of the Sraffa debate). To Hutchison, this paper

> . . . certainly marks a vital turning-point, or even U-turn, in Hayek's methodological ideas, and ought to be, but has not been, recognized as marking a fundamental shift in Austrian ideas. The main insights of this article are quite incompatible--except, possibly, at a level of extreme triviality--with the methodological ideas of his previous writings (1981, p. 215).

This "Hayek II" is the Hayek who is concerned with investigating the rules by which the free exercise of individual human faculties form a "spontaneous" social order. From 1937 onwards, this general preoccupation marks all of his work. In "Economics and Knowledge" (1937), his critique of the "Pure Logic of Choice" led him to redefine a social equilibrium as a compatibility of the individual plans that make it up. His later work (Hayek, 1973; 1976; 1979) follows this general methodological standpoint in investigating the analytical and moral implications of this "spontaneous order" by which free individuals allegedly construct a set of social rules out of chaos, according to the maxim, "The Results of Human Action, But Not of Human Design" (1969).

Hayek's influential methodological views are well-known to scholars of Austrian thought, but what seems to have escaped notice (except by Hutchinson) is the fact that his later methodological position implicitly rejects all of Hayek's earlier formal contributions to monetary and business-cycle theory. Interestingly for us, though, this brings his later work more closely in line, in terms of the methodological treatment

of money as a social institution, with Keynes' analysis of monetary theory that we have described above. Perhaps Hayek finally took Sraffa's criticism to heart after seeing the effective use made of the own-rates conception in the General Theory!

Two examples of the meaning of Hayek's later monetary views for his treatment of money illustrate this theme. First, in pursuing his critique of equilibrium states as tautologies of logic in 1937, Hayek makes note of the fact that the compatibility of individual plans, the only meaning he claims to see in the idea of a social equilibrium (1937, pp. 53-54), means that the shared mechanisms of society are (at least partially) defined by the attitudes individuals take toward them (pp. 62-67). It is on the basis of this notion that Hayek bases his fundamental distinction between subjective data and objective facts (pp. 51-53).

In the case of money, this idea can be very fruitful, although Hayek doesn't seem to have made any use of it in that direction. As Keynes showed, it is precisely the subjective attitudes of individuals toward the liquidity properties that are attached to the medium of exchange that gives money its peculiar force in economic life. But Keynes goes further than Hayek in this respect by analyzing the external effects that the spontaneous order of a socially defined liquidity standard imply for the pace of investment and employment.

It is notable that, although this subjectivist view of money fits easily into Keynes' monetary world of uncertainty and convention, this is a very different type of money than would have fulfilled the role of money in Hayek's Wicksellian business-cycle theory. In that case, the

goal was to eliminate, through the "neutrality" approach, any influence of monetary factors upon the otherwise smoothly operating equilibrium production of goods. But apart from the fact that Hayek has now (as of 1937) abandoned that view of equilibrium, if his insight into the subjective nature of money as a social institution is fully thought out (as in the General Theory, chapter 17), it becomes clear that the influence of money is not so easily done away with. This is precisely the meaning of Keynes' warning that his theory of shifting equilibrium must start from a monetary economy, and that the characteristic problems of a monetary economy could not even be escaped by abolishing "gold and silver and legal tender instruments" (1936, pp. 293-294).

A second concern of Hayek's in "Economics and Knowledge" that is very much illuminated by Keynes' shifting equilibrium theory is the concern Hayek expresses to include the expectations of individuals in the definition of equilibrium. Recall that it was exactly this point that Keynes stressed in defining his concept ("a system in which changing views about the future are capable of influencing the present situation"). Again, though, we see Keynes making much more of the notion than Hayek ever did.

Hayek wanted to define his "social equilibrium" as a compatibility of plans:

> For a society then we can speak of a state of equilibrium at a point of time--but it means only that compatibility exists between the different plans which the individuals composing it have made for action in time. And equilibrium will continue, once it exists, so long as the external data correspond to the common expectations of all the members of society (1937, p. 53).

But in terms of Keynes' vision of the complex balancing that equilibrium at a point in time represents, Hayek has got two things confused here. While it is true that it is the interaction of individual views about the future that give definition to certain aspects of equilibrium today, this is not the same thing as saying that all future plans are compatible. In fact, Keynes' analysis of the influence of expectations on the asset market showed the vital importance of a variety of views of the future--vital if the equilibrium was to be at all stable. Moreover, in Keynes' shifting equilibrium, the actual future content of the individual (investors', savers', speculators') expectations really mattered very little, since their effect would be felt today whether they were realized or not. This vital aspect of his view of an uncertain future is the basis both of his view of the social disutility of savings and the peculiar psychology of liquidity and money.

In relation to this last point, it is interesting to speculate on the extent to which the influence of Bohm-Bawerkian temporal analysis may be the crucial point of difference between Keynes and the Austrians. It has long been the lament of Austrians that Keynes' major failing as a theorist was a lack of understanding of capital theory. Hayek has made this criticism of both the Treatise and the General Theory and modern Austrians repeat it to this day. Yet, we have seen above that, contrary to these assertions, Keynes actually seems to have thought long and hard about capital theory problems and in fact to have based his analysis of interest and money on his own view of capital theory. Most likely the trouble for the Austrians has always been that Keynes (like many others)

explicitly did not find the traditionally Austrian emphasis on the time structure of production as a fruitful approach to the problems of capital theory. He specifically addresses this issue in Chapter 16 of the General Theory.

Briefly, his approach was to emphasize the reproducibility of capital goods and so to seek their value in their relative scarcity as a good. His famous barb to the Bohm-Bawerkian conception about "smelly processes," etc. was designed to illustrate the fact that while the productivity associated with a lengthening time structure of production is (sometimes) one aspect of the demand for capital goods, it is by no means the only one. In fact, Keynes seems to have been much ahead of his time in this respect since he explicitly noted that there may be conditions in which the extra roundaboutedness of production would not necessarily increase the value of the capital so employed. Echoes of the Cambridge reswitching controversies abound in his treatment of both this issue and of aggregate measures of capital.

But to get back to the definition of expectations-based equilibriums, it is obvious that the tremendous influence of Bohm-Bawerk on both Wicksell and Mises, and then Hayek, caused a disproportionate preoccupation by these authors with the time structure of production. G. L. S. Shackle makes this point in relation to Hayek in a synopsis of his contribution to economics:

> In reading The Pure Theory of Capital one is likely, I think, to conjecture that Hayek very early in his scholar's life gave his allegiance to Bohm-Bawerk and to Wicksell, that his mind was seized by an arresting idea, namely, that in investigating the nature of capital we are investigating an aspect of the nature of time. Such

> a conviction gives an immense impulse to intellectual effort, for it removes at one stroke any fear that the problem in hand may prove trivial or peripheral. . . . Some such influence seems needed to explain the huge effort which Hayek devoted to refounding the Bohm-Bawerk-Wicksell theory of capital (1981, p. 249).

And, of course, this devotion by Austrians (young and old) to such a capital concept could explain a lot about their peculiar hostility to a book which in fact tries to show that this element of time <u>is</u> peripheral to capital theory. For where Hayek may have been seized with the majesty of time, from Keynes' standpoint he failed to grasp its essence as a one-way, irreversible continuum. It may be the case that the influence of Bohm-Bawerk in this respect was actually to trivialize time by making it seem analyzable objectively, forwards or backwards, just like any other economic quantity.[6] Surely this is what led Hayek to treat the temporal aspects of goods as easily subsumed into an "intertemporal equilibrium" in 1928. Likewise, his business-cycle theory, based as it is on the "forced" lengthening and consequent crisis-induced shortening of the production process, treats the time element as a reversible factor. What we are arguing here is that this same point of view towards the nature of time pervades even his expectations-based equilibrium of plans in "Economics and Knowledge."[7]

If this speculation has any basis, then it goes far in explaining why Keynes' "shifting equilibrium" is so much more powerful an approach than Hayek's "compatibility of plans" theory. In Keynes' conception the fact that plans in the future may not be compatible or that plans made in the past are disappointed does not, in and of themselves, affect the equilibrium. What matters is the effect on currently held expectations.

Whatever their basis may be (and he implied with his analysis of conventional judgment, animal spirits, etc., that this basis may be fairly flimsy), to the extent that subjective plans influence current decisions (to save, invest, hold money, etc.) they are operative factors. Once time moves on, these expectations surely might be disappointed, but new expectations will necessarily be formed and they will be the important operative factor, not the (however influential) formerly disappointed ones.

One further insight this discussion yields up is an explanation of Keynes' abandonment (after some flirtation in the early work on the General Theory[8]) of any type of "period analysis" in his search for a new framework for employment theory. As evidence of Keynes' views on this subject, we have his exchange of letters with Bertil Ohlin and his subsequent note in the Economic Journal for December of 1937 on "The 'Ex Ante' Theory of The Rate of Interest" (all reprinted in Keynes (1973b), pp. 184-201, and 215-223). The relevant point to our discussion from these writings is that Keynes bases his general methodological argument against the use of a "periods analysis" on the treatment of time and expectations that such a conception implies. In a letter to Ohlin (1973b, p. 185), he clearly expresses this point:

> . . . when one comes to prove something truly logical and properly watertight, then I believe there are advantages in my method and that the ex post and ex ante device cannot be precisely stated without very cumbrous devices. I used to speak of the period between expectation and results as 'funnels of process,' but the fact that the funnels are all of different lengths and overlap one another meant that at any given time there was no aggregate realized result capable of being compared with some aggregate expectation at some earlier date.

In other words, it is not possible to logically treat current expectations as theoretically comparable to past expectations. What is relevant to decisions (and effective demand) is only the current period expectations. From the standpoint of an uncertain future, there is not much hope of all of these subjective plans ever coming into coordination or compatibility, as the Austro-Wicksellians would have it. Instead, what we should look for theoretically is a mechanism by which these diverse current estimates of the uncertain future are brought into equilibrium (however "shifting") today. This is one superiority of the own-rates framework when compared with the Swedish and Austrian versions of business cycle theory.

To bring us back to the connection with Bohm-Bawerk, it is important to reemphasize that a basic difference here is the theoretical treatment of time. In the Bohm-Bawerkian conception, where time takes on a physical characteristic that is no different from the other objective qualities of goods, it is a logical step to subsume time as one more element in the general equilibrium construct:

> For Bohm-Bawerk had drawn the rate of interest into the scheme of value-theory and encompassed it into the General Equilibrium system. He had bound it into the system of deliberate, voluntary, pre-reconciled and fully-informed actions by which all conduct can be explained as the response of reason to circumstance. Within such a system, there can be no involuntary acts, in particular no involuntary unemployment (G. L. S. Shackle, 1974, p. 53).

As with Bohm-Bawerk, this ill-conceived treatment of time, interest, and money seems to have continued to haunt Hayek's work. It was most clearly evident in his 1928 conception of an intertemporal equilibrium

system. It pervaded his Prices and Production, where crises resulted from the inequality between a previously expected time-structure of production and a currently expected one. And even after having thrown over the economic equilibrium construct as sterile, his conception of a prior compatibility of plans was still Bohm-Bawerkian in its treatment of time. Keynes, on the other hand, starting from the uncertain world of his monetary theory, was able to overcome this hurdle. In so doing, he was able to preserve those two cardinal starting points of economic theory, equilibrium analysis and individual rationality, while creating an analytical explanation of involuntary unemployment.

IV. INTERTEMPORAL EQUILIBRIUM RECONSIDERED

Given the tremendous ferment in monetary theory in the 1930s, particularly the tremendous attention given by all of the schools we have discussed (Austrian, Swedish, Cantabridgian) to the proper methodological framework for the analysis of a monetary economy, it is somehow surprising that the dominant approach in post-war monetary theory would be a reversion to the static Walrasian equilibrium system. Of course, from another standpoint, this should not be surprising (as we saw in Essay I). Since its reintroduction into the mainstream of Western economic thought by Hicks, Value and Capital (1946), the Walrasian paradigm, by virtue of its all-encompassing generality and the economic profession's demonstrated commitment to this abstract standard of the physical sciences, has successively conquered almost every theoretical domain of the discipline. One after another, its basic organizing principles were applied to value and distribution theory, macroeconomic theory, international trade theory and now even to applied policy studies. But, from the standpoint of monetary theory, the surprise should still linger. By general admission, it is the case that this framework has proved substantially useless in accounting for the social contrivance of money.

Incompatibility between the Walrasian paradigm and monetary theory reveals itself in a number of unresolved controversies. Way back at its inception, it was Walras' own never-fulfilled desire to incorporate the functions of money and credit into his system of simultaneous equations.

He only got so far as the incomplete consideration of the problems of admitting change into his system in the famous "Coda" to the Pure Theory. Walras' eminent disciple Knut Wicksell was stout enough of heart to undertake the formidable task of combining Walras' equilibrium with Bohm-Bawerk's capital theory, a task whose puzzles still elude us. Yet, when Wicksell came to discuss money and credit in volume two of his Lectures, the stringent formality of the general equilibrium setting was thrown over for a more informal disequilibrium, discussed in terms of an unspecified natural rate, a general price level and a cumulative process of inflation. More recently we have the example, duly noted in all monetary theory texts, of the fundamental incompatibility of Say's Law, Walras' Law and the Equation of Exchange when framed together in a general equilibrium setting. It was substantially in reaction to this incompatibility that the modern school of disequilibrium macroeconomics was started by Clower and Leijonhufvud in the late sixties. Thus, from the standpoint of the most recent work on monetary theory, we have come back full circle to Wicksell's treatment of monetary phenomena as disturbances to an otherwise equilibrium barter system. Again, the reason is the inability of the Walrasian system to account for money as anything other than an ad hoc afterthought. Frank Hahn, a true believer of the Walrasian faith, makes this point often. His attitude (1982, p. 1) succinctly sums up the state of Walrasian monetary theory:

> The most serious challenge that the existence of money poses to the theorists is this: the best developed model of the economy cannot find room for it. The best developed model is, of course, the Arrow-Debreu version of a Walrasian general equilibrium. A world in which all conceivable contingent futures contracts are possible

> neither needs nor wants intrinsically worthless money. A first, and to a fastidious theorist difficult, task is to find an alternative construction without sacrificing the clarity and logical coherence that are such outstanding features of Arrow-Debreu.

One is forced to wonder what is so "well-developed" about a model of the capitalist economy that can find no room for money and conversely what it is that is seen with such clarity by Arrow and Debreu. Hopefully, we can shed at least a glimmer of light on this wonder.

As we have seen above, one version of the general equilibrium approach that has its roots in those controversies of the thirties is the concept of an "intertemporal price system." In terms of the historical record, we know this is an outgrowth of the Austro-Wicksellian business cycle approach which, in turn, was a complicated brew of Bohm-Bawerk's capital theory with Walras' price system. In Hayek's original article (1928), he notes that then existing theory really had no formal apparatus for "assessing different prices of the same goods at different points in time." As a preface to his attempt to provide such an apparatus, he notes that a "sole exception to this is provided by the well-known works of E. von Böhm-Bawerk." But Bohm-Bawerk, as well as the others mentioned in this context (F. A. Fetter, K. Wicksell, I. Fisher and L. Mises), "though they are replete with hints of this kind, . . . have little to offer in the way of positive conclusions for the problems to be dealt with here" (1928, pp. 72-73).

But even if Hayek started from a Bohm-Bawerkian hint, the major influence of this idea on contemporary economic theory has been through its use in the Walrasian framework. Milgate (1982, pp. 125-142) offers a

good historical sketch of the path the concept took from its inception, in the work of Hayek and Lindahl, and then through Hicks (1946) and Malinvaud (1953; 1960-61), to the modern general equilibrium literature starting with Debreu (1959). For our present purposes, it is sufficient to note that the historical record shows the ease with which the treatment of "prices over time" fits into the conceptually static system of Walrasian general equilibrium theory. Again, the Bohm-Bawerkian treatment of time seems to be a factor, as Hayek's work illustrates.

The question of more pressing interest to our present study than the historical evolution of what Milgate calls "the method of intertemporal equilibrium" is the extent to which this approach to resource allocation over time compares to Keynes' own-rate framework of analysis. On prima facie grounds, the two concepts seem to have much in common. Both deal with prices of goods over time that define implicit interest rates. Thus, the question that arises is whether or not Keynes' own-rate framework can be usefully viewed as just a special case of that supremely general organizing framework, the Walrasian equilibrium system. If it can, there may be some credence to the claims of the Walrasians that their system is conterminous with economics, with even the treatment of money and interest where it has so far failed. Debreu himself expressed such a confidence in the generality of the intertemporal equilibrium framework in 1959:

> By focusing attention on change of dates one obtains, as a particular case of the general theory of commodities . . ., a theory of saving, investment, capital, and interest. Similarly by focusing attention on changes of locations one obtains as another particular

> case of the same general theory, a theory of location, transportation, international trade and exchange (1959, p. 32).

Our question will be whether this view represents the justifiably supreme confidence of the scientist who, upon climbing to the peak of his subject, can now gaze benignly at the lesser heights of the foothills below; or, whether, instead, this confidence is of the type engendered by a narrow mathematician staring too, too long at a complex, flickering image in a dark cave, until finally coming to take its image for the reality of the world outside.

Intertemporal equilibrium builds on and utilizes the sparse assumptions and lack of institutional detail of the general solution to the allocation of goods implied by the static Walrasian equilibrium. What it builds on is an amazingly simple extension for so large a task as encompassing the elements of capital-using production, time, and money into the basic supply and demand framework. Notably, the extension is to allow goods to be characterized not just by quality but also by the time and space (which are identical concepts from this viewpoint) in which they are available (Debreu, 1959, pp. 29-30). What is utilized from the basic case, then, is virtually everything that is analytically important. By this we mean that the characterization of production sets, preference sets and continuity in behavior that are required for the static (atemporal) equilibrium to exist, carry over to the intertemporal case:

> There is no need at all to rehearse again the definition of equilibrium, the question of existence and the question of free goods: for it is already done. All that is involved is a new interpretation of the old model which, from the formal point of view, makes no difference. . . . The extension to the intertemporal

> model is in a sense no more than a greater license to the imagination (Bliss, 1975, p. 46).

The chimera that this license conjures up in the imagination of the general equilibrium theorists is a system of equilibrium prices, one for each good, for each time period. For those goods which are not currently produced but will be in the future, the assumption is that there exists a complete set of futures markets that sell every good, for delivery at every future date, from the beginning of our time period today to the (necessary for existence) end of the horizon T. So for each good ($i = 1, \ldots, n$), in each time period ($t = 1, \ldots, T$), we get a particular equilibrium price P_{it}, NT of them in all. By what is a simple formal extension, but an enormous conceptual one, the solution for an intertemporal price vector, the prices of each good in each period, is then assumed to be accomplished in one fell swoop. Firms are assumed to operate via intertemporal production plans, consumers draw up intertemporal consumption plans, both for the whole time horizon simultaneously. The (non-unique) set of prices that prereconciles these two sets is the solution to the problem. All prices are determined at once (Bliss, 1975, pp. 39-49). We can now show what it means to say that in such a system, interest rates are only an ancillary phenomenon, implicit in the equilibrium price vector. For it is only after the equilibrium solution has been obtained that we go back to construct "own-rates of interest" from the already solved-for prices. We will follow the excellent discussion by Bliss (1975, pp. 50-60) to which the reader is referred for more details.

The equilibrium "intertemporal price system" contains one price for each good in each period. Since there is only one set of prices, we can choose only one numeraire by which to define the set of relative price ratios for the whole horizon. Each P_{it} will be the value of good i in period t relative to the numeraire good (a particular good in a particular period). In essence, we are defining "real" commodity prices in relation to one unchanging anchor for the whole intertemporal price system.

To get at the idea of interest rates, we ask what commodity trading ratios will be for different goods over different time periods. In the simplest case, let good 1 in period 1 be the numeraire ($P_{11}=1$), and all other prices be measured relative to how much of each good will trade for one unit of good 1 in period 1. Now ask: what does the equilibrium price vector tell us concerning the ability of an actor to transform (trade) one unit of good 1 in period 1 for some amount of good i in period t? There will be some quantity of this future good, call it X_{it}, that will trade for one unit of good 1 in period 1. The unit price of i in t will be P_{it}, and since $P_{11}=1$, the equilibrium price vector implies that

$$P_{it} X_{it} = P_{11} = 1, \tag{1}$$

hence,

$$P_{it} = \frac{P_{11}}{X_{it}} = \frac{1}{X_{it}} . \tag{2}$$

We can use this formulation to better interpret what these

intertemporal prices are. From (2) we can see that each P_{it} in the equilibrium price vector can be interpreted as the inverse of the quantity of good i in period t that one unit of the numeraire will purchase. Thus, there will be an intertemporal exchange ratio, in quantity terms, implied by the relation of the equilibrium ("present value") prices to each other.

The barterlike rates of interest that we can derive from such a set of "intertemporal" relative prices are based on the necessary interrelationships between these prices. Ask: how much extra good i could be obtained by deferring the delivery of good i from week t to week (t+s)? Denote this intertemporal exchange ratio as $\rho_i(t,t+s)$, the own-rate of interest on good i for the period t to t+s. Then, if the present value prices imply

$$P_{it} = X_{i,t+s} P_{i,t+s} ,$$

then the "own-rate" of interest will measure the proportionate gain in terms of good i over the period t to t+s:

$$\rho_i(t,t+s) = X_{i,t+s} - 1 .$$

Since by (1)

$$X_{i,t+s} = \frac{P_{i,t}}{P_{i,t+s}}$$

we define these own-rates as,

$$\rho_i(t,t+s) = \frac{P_{i,t}}{P_{i,t+s}} - 1 = \frac{P_{i,t} - P_{i,t+s}}{P_{i,t+s}} . \qquad (3)$$

Thus, the own-rate of interest "is measured by the rate of decline of the present-value price expressed as a proportion of the price of the later period" (Bliss, 1975, p. 52).

This is the bare-bones of the intertemporal equilibrium construct. Our question is: How does it compare to the own-rates framework developed by Sraffa and Keynes? Remembering the clear distinction between these two (Sraffa's and Keynes') uses of own-rates, now that we are presented with a third use of the term, it will be convenient to adopt a policy to distinguish them. To this end, we will henceforth denote Sraffa's conception as "long-period commodity rates" and represent them with the term r_c. Alternatively, we will call Keynes' most elaborate concept (recalling that he recognized and built from Sraffa's usage) "money own rates," and represent these as $r_{c,m}$. Finally, we will retain $\rho_i(t,t+s)$ as our symbol for the own-rates of the Walrasian intertemporal equilibrium and call these "short-period commodity rates." The reason for choosing these terms will become more evident as we proceed. Basically, our comparison will involve the question of the conception of equilibrium which underlies these respective uses. The analysis starts naturally from a comparison of the different discussions of the own-rate of interest, beginning with Sraffa's.

Sraffa's long-period commodity rates are defined by his conception of "equilibrium" (in all of his writings it would seem) as corresponding to the long-period classical conception. Since we have already discussed this concept, it is only necessary to show that the traditional long-period equilibrium prices imply Sraffa's long-period equilibrium own-

rates. In the position of long-period equilibrium, "natural" prices reflect a composition of resource allocation such that each price reflects an equal return to producers on the value of their means of production (see Garegnani, 1976, pp. 26-29). As Sraffa notes, this implies that all prices equal costs of production and that any deviations from this "position" of the economy imply differential rates in different industries. The unchanging nature of this type of equilibrium was illustrated by Joan Robinson's (1953-54, p. 85) famous quip:

> But it is impossible for a system to get into a position of [long-period] equilibrium, for the very nature of equilibrium is that the system is already in it, and has been in it for a certain length of past time.

Without entering into the extended modern debate over the use of the long-period conception by "supply and demand" theories versus "classical theories" (see Garegnani, 1976; Milgate, 1983; Robinson, 1981), it is clear that for whatever reason (as a purely analytical distinction, or the result of a real process), long-period equilibrium prices would be either unchanging or at least absolutely predictable and steadily changing. The distinction concerns the more narrow question of whether the long-period need be a static stationary state, or one in which capital accumulation proceeds, but at a long-period rate (see Robbins, 1930; Schumpeter, 1934; Samuelson, 1943; Milgate, 1982, pp. 28-33).[9] To Sraffa's case, though, it makes little difference since he defined as "equilibrium" long-period commodity rates a situation in which all "spot and future prices coincide," and all "'natural' or commodity rates are equal to one another, and to the money rate." That the money

rate is even included in the definition is just a bow to the use of the term "money" in such a long-period barter system for the accounting numeraire. As Sraffa pointed out, the whole framework precluded any discussion of the more realistic aspects of a money economy from the start. But in following out such a conception, his permanent criticism of the Wicksellian natural rate was on the internally secure grounds of accepting Wicksell's own concern with defining long-period equilibrium states (Wicksell, 1935, vol. I, p. 97, pp. 155-156, p. 166).

Notable in Sraffa's long-period conception is the fact that equilibrium is defined by the distribution of capital resources to each industry, as befits the classical long period. Thus, his long-period prices are "cost of production prices" where a general rate of profit is earned on each:

> It will be noticed that, under free competition, this divergence of rates [when out of long-period equilibrium] is as essential to the effecting of the transition [between long period positions] as is the divergence of prices from costs of production; it is, in fact, another aspect of the same thing (Sraffa, 1932a, p. 50).

It is from this long-period conception of equilibrium that his long-period "equilibrium" own-rates arise. Since, by definition, long-period prices are fixed ("centers of gravitation") positions defined by the technical conditions of production, then spot and future prices will coincide and all own-rates will equal the money-rate. This seems to be the meaning of equilibrium behind Sraffa's use of own-rates of interest. All r_c^i equal for every good ($i = 1, \ldots, n$).

Before we leave Sraffa, it is interesting to note that there are some conceptual problems skirted by his discussion, and more

interestingly that these problems might be addressed by an analytical combination of his later work on classical value theory (1960) with Keynes' own-rates framework. Notice that Sraffa's definition skirts two issues. First, there is no discussion of what might determine the common natural-rate or commodity-rate of interest. And secondly, he makes no mention of what level this common rate will settle at. These are closely linked ideas concerning the definition of the "natural rate" of interest. Again, the view of Sraffa's review as immanent criticism of Wicksell is useful in this connection, since these questions in fact remained open as part of Wicksell's capital theory legacy. Wicksell thought "real" conditions of productivity, operating through an aggregate conception of capital in long period conditions, determined the "natural-rate" (see Wicksell, 1935, vol. I). Of more relevance to Sraffa's review is that Wicksell clearly distinguished between this real, barter-like rate, and the money rate established in credit markets. What Sraffa is criticizing is not the underlying conception of an equal long-period rate of return (indeed, he spent most of his life's work trying to give this concept a better analytical basis), but the separation between real and monetary forces based on the distinction between a barter-like real rate and an observed money rate.

Later, though, Sraffa went further by criticizing the whole idea of an aggregate concept of capital on which this natural rate would be determined (1960). In this later conception, he provides a system by which the general long-period rate can be determined without an aggregate view of capital. Yet, the further question of what level this rate will

be set at is "left open" by Sraffa's (1960) system. And what is more, he hints that it may be tied down by a consideration of the money rate. This is where Keynes might come in--but first we must return to Walras.

In the intertemporal price system, proposed by Hayek and developed by the modern Walrasians, the whole long-period basis of equilibrium is lost. This is clearly reflected in the definition of the short-period own-rates such a system defines. Since the Walrasian short-period rates are unconnected to any conception of competition as leveling rates of return in different sectors, there is no reason to ever expect the $\rho_i(t,t+s)$'s to equalize. Bliss (1975, p. 55) makes explicit note of this:

> Notice the special assumptions that are necessary for an intertemporal equilibrium to have associated with it a single constant rate of interest. In general, all that is known is that in any week there is a uniquely defined own-rate of interest for any good, but this may differ from the own-rates of interest for other goods in that week or from the own-rates of interest for that good in a different week.

As we have seen, Sraffa also recognized the existence of commodity own-rates which diverge from each other. But he identified them as <u>disequilibrium</u> rates in the long-period sense, since they necessarily represented differential profit opportunities for different goods. What the intertemporal equilibrium theory does is to elevate this short-period situation to the definition of a putative "general" case of equilibrium. This difference in "vision" of the economic process reveals itself in two other ways than just the divergence of own-rates in equilibrium. The very concepts of prices and interest rates are different in each.

First, the intertemporal equilibrium abstracts away from

descriptions of how the flows of resources to particular industries might be regulated at a social level. In the place of the general rate of profit that governs this flow in the long-period conception, the intertemporal equilibrium approach substitutes the individual supply and demand decisions in each market in each time period. Accordingly, where Sraffa's long-period prices were constant cost of production prices that reflected the general rate of profit, in the intertemporal case each P_{it} is just one of the system of market prices between which there is no a priori expected relationship. Thus, the two equilibriums each yield a conceptually different set of prices and consequently the own-rates derived from these prices in each are conceptually different. In Sraffa's long period, divergent own-rates have the function of signaling the flow of resources out of low profit sectors and into high profit ones. In the intertemporal case, the own-rates have no signaling function at all, except to express intertemporal exchange ratios in a convenient form after they have already been determined. The conclusion: own-rates are of no analytical importance in the intertemporal approach.

> Thus the central point is that interest rate calculations are ancillary to the analysis of prices in a model in which the allocation of given resources is viewed over a sequence of time periods. . . . We therefore conclude that because interest rates in a multiperiod model for the theory are implicit in the solution for prices, no essentially new insight is gained by restating the price constraints in terms of these interest rates (Walsh and Gram, 1980, p. 236).

But the long-period conception of equal commodity rates and the Walrasian conception of divergent short-period own-rates share with each other a deficient treatment of money. Both are essentially barter

frameworks. As we know from Sraffa's review of the Wicksellian natural-rate framework, he didn't think the approach left any room for the social character of money. The intertemporal equilibrium construct, likewise, can perform all of the tasks it is designed for without money. When a market exists for every good, present and future; when a prior compatibility of supply and demand in each market is assured by the definition of equilibrium; and when actors make all plans for their whole time horizon simultaneously, what is there left for money to do?

This compatibility of the two barter conceptions in rejecting money as necessary to the formation of equilibrium opens the way to a comparison with Keynes' own-rates framework. Keynes' definition of equilibrium monetary own-rates of interest adapts the long-period vision of capital flows to the situation of a monetary economy where the Walrasian treatment of time makes no sense and the future is incalculably uncertain. In Keynes' conception, monetary own-rates forge a connecting link between the social contrivance of money as a liquidity standard and the effects of competing wealth holders on the flow of resources. The end result, as we know, is a theory of unemployment equilibriums.

In terms of the ability to account for money, the crucial difference between Keynes' monetary own-rates and the barter own-rates is the recognition of uncertainty about the future that cannot be built into another quality of future goods. Since Keynes' actors do not have a market for each future good which they can rely on (either probabilistically or certainly), they demand liquid wealth holdings. Liquidity premiums are paid by individuals for goods that possess a

potential power of disposability over an unspecified period of time. Thus, the consideration of time here is an open-ended one, not a fixed time horizon as in the intertemporal case. Further, it is when we admit the rational response by actors to this view of time that the importance of expectations in Keynes' shifting equilibrium comes to the front. In the intertemporal case, the conception of time precluded the existence of Keynesian expectations by making future markets equivalent to present ones. When this fantasy is pulled away, room opens up for the liquidity functions of money.

In terms of the definition of equilibrium own-rates, the monetary character of Keynes' vision, and its distinction from the intertemporal equilibrium approach, reveals itself in a number of ways. First, Keynes' system is completely defined by transactions that take place today in expectation of the uncertain future. Thus, not only does his account involve expected values of the individual capital and financial assets, but the monetary standard in which they are measured is captured by his "expected appreciation" terms, the "a's." Note that these "a" terms are absent from the intertemporal own-rates since each present value price is defined by a single unchanging standard, the numeraire good. Moreover, the liquidity premium for money assets is absent from this framework because the conception of time as space in the intertemporal approach precludes the need for liquidity. Finally, the very conception of how we might "get into" equilibrium is given a much more "feet-on-the-ground" approach in Keynes' vision than in the Walrasian one.

Recall that the equilibrium prices in the Walrasian framework

reflect an idealized prereconciliation of every plan, of every individual, for the whole time horizon. This is the function of the fictitious "auctioneer" mechanism by which no trades are allowed until this configuration of plans has been achieved. We argued in Essay I that this auctioneer process masks a tremendous deficiency of the Walrasian approach; i.e., that there was no description of how a Walrasian equilibrium might be achieved. Coming out of the Marshallian tradition, Keynes was more alive to grounding even his most abstract discussions in some reference to institutional realism. In the case of the own-rates framework, this realism provides him with a treatment of the formation of the equilibrium on the asset market that seems much more plausible than the Walrasian auctioneer.

Essentially what ensures the fragile but continuous existence of Keynes' equilibrium monetary own-rates is the self-motivated actions of wealth owners seeking the highest financial return for their assets. With his wide experience in financial markets, it was natural for Keynes to utilize the arbitrage process, which works so tropismatically in asset markets, as the driving force of his "equilibrium." Of course, as he also noted (1936, Ch. 12), the existence of a professional class of investors who provided this equilibrating function might also account for the fragile nature of his shifting equilibrium. In any case, Keynes' equilibrium own-rates have a practical basis for existence that represents a large improvement over the extraordinary fiction of the Walrasian auctioneer (especially in the intertemporal setting). Not only does the arbitrage mechanism provide a social level mechanism by which

individual preferences are reconciled, but it does so by appeal to good old Smithian self-interest.

On the other side of this asset market equilibrium from its underlying micro rationale, we find addressed the concern of the long-period approach with the effect of such self-interested investment on resource flows. Again, the innovation of Keynes' approach is to include monetary considerations in his vision from the very start. By the classical long-period conception that Sraffa's commodity-rates discussion moved in, the resource flows and the natural price/market price deviations that these flows responded to, were conceived of in strictly "real" terms. This is why for Ricardo no less than for Wicksell, despite widely different theories of value, monetary effects were seen as disturbances, disequilibrium effects and transitory phenomena. In Keynes' shifting equilibrium, we alter this view by including the influence of money from the start. Thus, the asset market equilibrium determines equal <u>monetary</u> own-rates at any particular moment for all assets, real and financial. In Keynes' equilibrium, $r^{i}_{c,m}$ are equal for all goods ($i = 1, \ldots, n$), but all r_c may differ. The difference between the two is accounted for by the expected appreciation terms, the "a's".[10] Those assets prices so determined will govern the flow of production of new capital goods when compared with costs of production. We can now see that the basis of Keynes' treatment of money as both just another asset and one with very peculiar capital qualities is his need to bring monetary considerations into his theory on equal footing with productivity considerations. Further, this equality of financial and

capital assets in the eyes of the wealth holders leads him naturally to consider a common basis for all assets--hence his capital theory grounded in reproducibility and scarcity. Finally, it is the monetary character of even this, his most elaborate, model (that of Chapter 17) that allows him to define unemployment equilibriums that are the result of persistent forces. In an uncertain monetary economy, the private need for a social standard of liquidity, as a result of human action but not of human design, has the potential to regulate the utilization level of resources as a by-product of directing their allocation.

In bringing these dilatory remarks to an end, it is convenient to recall that we began this section with the question of the all-encompassing generality of the Walrasian paradigm. In the remarks of Debreu, we saw a commonly held notion that the sparseness of institutional detail and the abstract generality of the full intertemporal general equilibrium theory could encompass any particular theory ("of savings, investment, capital and interest") as a special case. By this reasoning, a single framework is offered for the analysis of all economic problems. Now, having reviewed three different conceptions of equilibrium as they relate to the idea of own-rates of interest, can we agree with this viewpoint?

In framing our answer, it would seem that the crucial question is the grounds on which generality is claimed in each case. On the grounds of a theory of the allocation of a given amount of resources by decentralized decisions, expressed through idealized markets, in a setting of perfect foresight, the Arrow-Debreu system seems to have a

just claim to all-encompassing generality. But Robbins' famous definition notwithstanding, one could conceive of other possible standards by which to judge a vision of the economy than hypothetical allocational efficiency. In fact, this seems to be exactly what Keynes did in claiming a generality for his theory over the classical one. In other words, he changed the object of his theoretical enquiry to include the two things that the Walrasian theory seems incapable of admitting, short of an ad hoc afterthought or restriction: the use and effects of a socially defined medium of exchange and the existence of unemployment equilibriums. Are these a foothill seen far below from the abstract heights of the Arrow-Debreu economy? Perhaps the general equilibrium theorist, having ground his nose to rock and having finally achieved his summit, will now turn around to find a never-before-glimpsed range off in the distance. Which is higher?

V. ENDNOTES

1. In fleshing out his conception of divergent own-rates of interest as being a corollary phenomenon to divergences between market prices and long-period "natural" prices, Sraffa makes the long-period context of his argument quite clear. See Sraffa (1932a, p. 50).

2. The distinction involves the more narrow question of whether the long-period definition of prices and (equal) profit rates necessarily implies a "stationary state" condition. Many authors have noticed that the neoclassical use of the long-period (e.g., by Wicksell, Clark, and Marshall), is usually conceived of as a theoretical state in which accumulation is still going forward--in other words, a nonstationary long-period. Since the actual defining point of the long-period approach concerns the leveling of rates of return, it is obvious that such a dynamic long-period as Marshall proposed must be a form of what modern economists call a steady state. Milgate (1982, pp. 28-33) covers this topic exhaustively and contains all of the relevant references. It is reported that much is made of this idea by someone working in Montreal, Canada.

3. "I can date my own personal 'revolution' rather exactly to May or June 1933. . . . It began (rather oddly, as it turned out) with Hayek" (Hicks, 1963, p. 307). See also Hicks (1973, p. 190) where he traces this influence of Hayek to his Value and Capital.

4. Although for a much earlier recognition of the different conceptions of equilibrium implicit in various approaches to economics,

see Robbins (1930).

5. That Hayek is in fact identifying his approach to "equilibrium" with the Walrasian approach is clear as Hutchison (1981, p. 212) shows with the following quote from Hayek (1949, p. 42n). Hayek claims that by "equilibrium theory":

> We have primarily understood the modern theory of the general interdependence of all economic quantities, which has been most perfectly expressed by the Lausanne School of theoretical economics.

Hayek's early self-identification with the static Walrasian equilibrium approach would seem to present a severe difficulty for the attempt by the modern Austrian school to interpret all of his work as part of "a school apart" which concentrates on subjective process analysis as opposed to the specification of equilibrium states. For instance, one of Hayek's most cogent modern supporters makes the following claim in this regard:

> Throughout the 1930s and 1940s, Hayek was a major critic of the emerging professional consensus on economic research. In particular, he tried to separate the theoretical from the empirical (as he phrased it) in economics and delimit the tautological propositions of economic analysis [i.e., General Equilibrium Systems] from the potentially empirical elements. He argued that the tendency to limit economic theory to the development of static analysis would make it impossible to deal with disequilibrium conditions. His arguments often anticipated current criticisms of the cavalier treatment of disequilibrium states by economists (O'Driscoll, 1977, p. 18).

As our study will show, while it is true that <u>sometimes</u> Hayek criticized "tautologous equilibrium states" in the 1930s and 1940s, he also embraced just the opposite methodological stance in the same period. Given that there is no unity in his methodological views, some other basis seems called for to connect his multifarious works.

6. This distinction between treating time as a physical characteristic ("time as space") versus treating time as a subjective life process has roots in the literature of phenomenological philosophy. More recently, much has been made of the same conceptual distinction (between "Newtonian" and "Historical" time) by the Post Keynesian theorists. See Kregel (1973, pp. 31-32) and Robinson (1981, pp. 86-95).

7. It is interesting to note in this context that in the most recent theoretical treatise of the modern Austrian school (O'Driscoll and Rizzo, 1985), an attempt is made to purge Austrian subjectivist views of the inadequate treatment of time involved in Hayek's "coordination of plans" equilibrium:

> Hayek's avowed intention in developing his concept of equilibrium as the consistency of individual plans was to marry time and equilibrium (Hayek, 1937, p. 37). Since plans are forward-looking, he reasoned that plan coordination must entail time. Unfortunately, he did not fully understand the distinction between the Newtonian and real-time constructs. Hayekian equilibrium incorporated only Newtonian time (p. 81).

8.

> As regards the ex post and the ex ante method, I shall certainly give further thought to its advantages. This is in fact almost precisely on the lines that I was thinking and lecturing somewhere about 1931 and 1932, and subsequently abandoned. My reason for giving it up was owing to my failure to establish any definite unit of time, and I found that that made very artificial any attempt to state the theory precisely. So, after writing out many chapters along what were evidently the Swedish lines, I scrapped the lot and felt that my new treatment was much safer and sounder from a logical point of view (Keynes to Ohlin, Jan. 1937; reprinted in Keynes, 1973b, p. 184).

9. See note #2 above.

10. See Essay II above, pp. 124-135.

A CONCLUDING WORD

In the Introduction to these Essays, an attempt was made to bring some coherence to the various themes, motivations and subjects treated in each. One claim made at that time for unity in these works was a common concern with the usefulness of historical interpretations of economic theory for modern understanding. Accordingly, we propose to conclude these Essays, not with a summing up of accomplished tasks, but with some brief remarks on the future avenues of research that these topics suggest to one student--viz., the author.

In the case of the own-rates theory of interest, there are a (painfully) large number of areas in which the above coverage, being merely suggestive, could be extended. We will touch on a few of them. Fisher's real rate was mentioned briefly in Essay II. Given the enormous practical and theoretical influence this concept has exercised from Fisher's day down to our own, it warrants closer attention if the own-rates might shed any light on its puzzles. As noted, Adarkar seemed to think that it did, and Keynes also makes some short comments on it. Utilizing the Sraffian conception of barter-rates, it seems likely that a ready empirical estimate of a number of actual "real-rates" could be found. These would include those for commodities which are traded on organized futures markets. These could then be usefully compared to a concurrently existing money rate to get a new angle on Fisher's effect.

Along the same lines, the own-rates theory suggests that a term-structure argument would be implicit in a comparison of the expected

rates of appreciation (the "a's") which go into the calculation of commodity own-rates, for different periods of time. This might be an interesting way to frame the traditional expectational theory of the term structure that has a more operational set of data points than the usual lagged expectations terms. A comparison of the term structure of different real own-rates and money rates would be interesting from this standpoint as well. Is there actually such a distinction in expectation?

More ambitious students of mathematics have an interesting subject in the possible specifications of the micro-structure of Keynes' asset-market equilibrium. Game theoretical account might be appropriate in this context since there seems to be room for discussing identifiable groups facing different objectives and constraints. Moreover, the second- (or third-, etc.) order guessing implied by the "beauty contest example" seems to be potentially amenable to this type of specification.

On the other side of the asset market from the micro-structure, the stock-flow relationship of asset prices and investment activity that Keynes makes so much of has also not ever been successfully addressed. Although many authors have attempted to write the last word on the limits of the stock-flow relationship over the years, until recently (Foley, 1975) none have made much sense of the interaction of the two. This is not surprising since the mathematicians report this to be one of the most vexing problems from a formal standpoint.

Finally, the most ambitious of all avenues of research suggested by the own-rates theory concerns the further specification of an alternative framework for value theory. Recalling Townshend's remarks, it seems that

one aspect of such an effort would have to accord the liquidity/uncertainty nexus of Keynes' monetary views an equal standing with the traditional focus on quantity relations. In other words, a theory of shifting equilibrium would have to build up to market prices that are only determined to the degree of stability that attaches to the money that measures them. An obvious avenue of distinction here would be the classification of goods by the degree to which they participate in a socially defined liquidity premium. How are such goods different and how do they arise and recede?

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